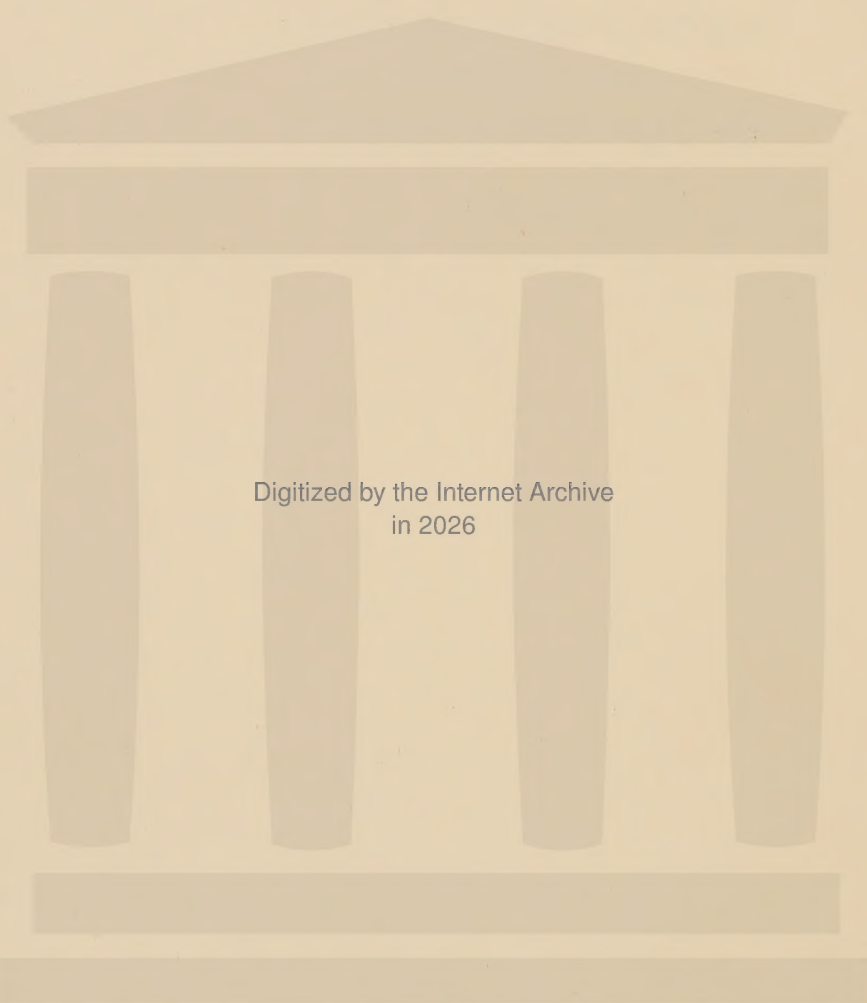


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BIOGRAPHICAL MEMOIRS
OF FELLOWS
OF THE ROYAL SOCIETY

VOLUME 4



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BIOGRAPHICAL MEMOIRS
OF FELLOWS
OF THE ROYAL SOCIETY

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A photograph of the subject is printed with each notice



W J Arkell

WILLIAM JOSCELYN ARKELL

1904-1958

W. J. ARKELL, the youngest of a family of seven, was born at Highworth, Wilts., on 9 June 1904. His father, James Arkell, was junior partner with his brother Thomas in Arkell's Brewery, Kingsdown, near Swindon, a prosperous business founded by their father, John Arkell. Thomas Arkell died in 1919 at the age of 84, and James (the youngest of a family of 13) then became head of the firm until his death in 1926 at the age of 74. The family is believed to have immigrated at the time of William of Orange, and to have taken its name, from Arkel, in Holland; and until John Arkell started the brewery business its members had been mostly farmers. W. J. Arkell's mother, Laura Jane Arkell, was one of four children of a London solicitor, Augustus William Rixon, all of whom attained the age of 85 or more. She was an artist of considerable ability, as was also her brother, W. A. Rixon, of Northleach.

A deeply rooted love for the English countryside influenced Arkell from an early age. Alone or with a brother he explored every land, spinney, hedge and ditch around Highworth, collecting insects, snails, plants and fossils. The Diptera were his special interest. The family summer holidays, always spent at Swanage, afforded opportunities for the Dorset coast and interior to be explored with equal thoroughness, on foot or by bicycle. He was sent in 1914 to Durlston Court preparatory school, Swanage, and later entered Wellington College, Berks. He does not appear to have displayed exceptional ability in any branch of ordinary school studies; having, moreover, outgrown his strength (he was 6 ft. 2 in. tall at the age of 13 and 6 ft. 4½ in. at 17½), he had little aptitude for games. Admission to the school Field Club (a somewhat exclusive body limited to 14 members) was, however, his consolation and salvation, for it carried with it the privilege of devoting games periods during the summer term to field work in natural history. His collection of Diptera grew rapidly, and, always very methodical, he entered his observations in copious diaries. In two successive years (1920 and 1921) he was awarded the school Pender Prize for the best essay in natural history. The first essay was a treatise on the Robber Flies (Asilidae) and the second on the natural history of the Dorset heaths; both were illustrated in colour. In his last summer at school (1921) he gained prizes for the best natural history diary of the year and the best display at the annual exhibition of the school Natural History Society. Few of his friends in later life were aware that while at school and during his earlier years at Oxford Arkell wrote a number of poems, some of which were published in *The Tower* (the Wellington

College magazine) and elsewhere. Several, composed at about the age of 17, are assembled in a small privately published booklet, *Seven Poems*, distributed to his friends posthumously on the 54th anniversary of his birth. These charming poems could only have been the work of one with a profound love of Nature and the countryside.

Arkell left school in December 1921, passed Responsions (but not until the second attempt owing to weakness in Latin) in the spring, and entered New College, Oxford, as a commoner in October 1922. By this time he had decided to make geology and palaeontology his career, and to relegate entomology to second place among his interests. This decision was reinforced by the inspiration and encouragement of that versatile enthusiast, W. J. Sollas, then Professor of Geology, although Arkell has left it on record that 'my biology tutor was Julian Huxley, who made zoology so interesting that he nearly weaned me from geology to it, but not quite'. In June 1925, he gained the only first-class honours of his year in geology, and in the following autumn sat for and was awarded the Burdett-Coutts Scholarship, which opened the way for research in his chosen field. The geology and palaeontology of the Corallian beds (to which the rocks around Highworth belonged that he had known since his boyhood) were the subject of his earliest post-graduate work. His first important paper, entitled 'The Corallian rocks of Oxford, Berks. and North Wilts.', was communicated by Sollas to the Royal Society in 1927, and publication of his 'Monograph of British Corallian Lamellibranchia', based on the thesis which gained him his D.Phil., was begun by the Palaeontographical Society in 1929.

At this same period an ideal opportunity arose of broadening his experience. For four winter seasons (1926-30) he joined K. S. Sandford in a survey of the traces of Palaeolithic man along some 1200 kilometres of the Nile Valley and much of the neighbouring country, organized by the Oriental Institute of the University of Chicago under the direction of Professor J. H. Breasted. Four notable monographs published by the Oriental Institute under the joint authorship of Sandford and Arkell were the main outcome of this work. Arkell always retained an interest both in prehistory and in the geology of Egypt, a country which he re-visited much later in life.

His Jurassic field-work was meanwhile being extended gradually to other parts of the system and to other regions. Summers were still spent on the Dorset coast and visits were paid to France and Germany, where museum specimens were examined and observations made in the field. In a paper published in 1930 he compared the Jurassic rocks of the Calvados coast of France with those of southern England. Collaboration with J. A. Douglas in a survey of the Cornbrash along its entire English outcrop led to two joint papers published by the Geological Society in 1928 and 1932, and study of the Great Oolite of Oxfordshire to a paper published in 1931 by the same Society. For some years Arkell had now been ambitiously assembling material for a major work, his *Jurassic System in Great Britain*, published in 1933. This was an amazing achievement for a man still in his twenties. An imposing

volume of 681 pages and copious illustrations, it presented a coherent, critical, and up-to-date survey of the subject, bringing together information scattered in hundreds of memoirs and papers, many in relatively inaccessible publications of provincial societies. He was on classical ground, for the rocks of which he wrote were those that had long previously been studied and grouped in formations by William Smith, the first to observe that such formations succeeded one another everywhere in the same order and that each could be identified by its characteristic fossils. In a well-written chapter (now widely used as a text for teaching) Arkell reviewed the history of the theory of classification and correlation of fossiliferous rocks from the time of Smith onwards, discussing in turn the stages of d'Orbigny, the zones of Oppel, and the hemerae of Buckman, and he pointed out the need for caution in accepting at least some of Buckman's published conclusions. In a further introductory section he demonstrated the relation between the distribution and facies of the rocks and troughs of sedimentation and axes of folding, while general palaeogeographical conclusions formed the subject of a final chapter. This work was very favourably reviewed by F. A. Bather, F. L. Kitchin, and others in this country, and by a number of writers abroad; it immediately gained for Arkell an international reputation as a foremost authority on Jurassic stratigraphy and palaeontology, and to the present day remains an indispensable work of reference.

His Burdett-Coutts Scholarship expired in 1927, and in 1929 Arkell was appointed a Lecturer at New College, Oxford; while in 1933 he was elected a Senior Research Fellow of the College, continuing as such until 1940. He was awarded the D.Sc. degree of the University of Oxford in 1934. He married, in 1929, Ruby Lilian Percival, only child of S. R. S. Percival, of Boscombe, Hants., and established a charming home at Cumnor, near Oxford, where many friends were hospitably received, as they were also at the bungalow at Ringstead Bay, Dorset, where he spent most vacations. Three sons, Raymond, Julian and Mervyn, were born in 1932, 1934 and 1937 respectively. In 1933 he visited the United States of America to attend the International Geological Congress, held at Washington, where he read a paper entitled 'Analysis of the Mesozoic and Cainozoic folding in England'. He took part in a Congress excursion of 1100 miles in New England and made many American friends.

With teaching duties that were never more than nominal and with no curatorial or other time-absorbing occupations, Arkell was fortunate in being able to devote almost all his time to research. Not a moment of that time was wasted. Having completed his monograph on Corallian lamelli-branches, he decided to turn to the study of ammonites as being more applicable to general problems of Jurassic correlation. The difficult literature of this group was mastered with his customary ability and thoroughness, and by 1935 the first annual instalment of a monograph of the ammonites of the English Corallian beds was ready for publication by the Palaeontographical Society. Ammonites from the Kimmeridge and Oxford Clays formed the

subjects of shorter papers. He still continued with detailed geological mapping in various areas. Among several papers of this period in which the results of his field work were recorded may be mentioned those on the Portland beds of the Dorset mainland (1935) and on the Corallian beds of Dorset (1936), and those explaining and including his maps of the Corallian beds between Marcham and Faringdon, Berks. (1939) and around High-worth (1941). The 'Coral Rag' of Berkshire and adjacent counties interested him particularly, since, during his visits to Egypt, he had been able to study the modern fringing reefs of the Red Sea littoral and their ecology. Its origin was discussed by him in a paper published in 1935, and the conclusion was reached that this formation, which occurs in irregular patches, represents a series of fringing reefs formed in a shallow sea and still in their position of growth. Between the patches of Coral Rag are strips of detrital limestone representing the current-bedded debris which filled the channels between the reefs. Arkell accepted the opinion of most previous writers that a tropical or subtropical climate existed in the north-west European area (and probably over the whole world) in Corallian times.

Tectonic phenomena also continued to interest him. A detailed study of the disturbances affecting the rocks of eastern Dorset led him to the conclusion that some existing ideas regarding the structure of the area were untenable. The Purbeck fault, in particular, considered since the time of Strahan to be a thrust-fault resulting from an overthrust from north to south, was shown to be a normal fault with a downthrow to the north, due most probably to adjustment of the beds following their folding. The disturbed condition of the beds in the Isle of Purbeck from the Purbeck series to the Lower Chalk was attributed to the adjustment of these incompetent strata between two folded competent formations, the Portland Stone and the White Chalk. These ideas were developed in papers published from 1936 to 1938. By 1939 an important memoir on the geology of the Weymouth district and the Isle of Purbeck had been completed with some co-operation by other workers, accepted for publication by the Geological Survey, and even set up in type. Its publication was interrupted by the outbreak of war, and it did not eventually appear until 1947. Reference may also be made here to a paper on the gastropods of the Purbeck beds published by Arkell in 1941. His illustrations of these small shells, like many of the figures in his other works, including the Weymouth memoir, testify to his ability as an artist. Painting and drawing were, in fact, favourite hobbies, and his watercolours of favourite scenes in Dorset and elsewhere are most pleasing.

In 1941 the war led Arkell to become a temporary civil servant. He took up an appointment as a Principal in the Economic and Inter-Allied Branch of the Ministry of Shipping (later Ministry of Transport), and went to live in London at a time when enemy bombing was intense and conditions of life very rigorous. Whether or not it was attributable to the strain of an existence so different from that to which he was accustomed it is impossible to say, but late in 1943 he suffered a breakdown in health and was admitted in a serious

condition to a sanatorium, where his life was just saved by a pneumothorax operation and five months were spent in all. He was unable to resume his Civil Service duties, and until 1945 was not well enough even to attempt geological field-work. For several years his indoor working hours had to be severely restricted, and he was never subsequently a really fit man.

Arkell's election to the Royal Society took place in 1947. In the same year he was offered a University Lectureship in the Department of Geology at Cambridge, but had to decline owing to his poor state of health. He then received and accepted with enthusiasm the offer of a Senior Research Fellowship at Trinity College, Cambridge. A study was allotted to him on the ground floor of the Sedgwick Museum, where he was welcomed cordially by his new colleagues and continued with his palaeontological work. After living in rooms in College for a year he moved his home to Cranmer Road, Cambridge, in 1948. His departure from Oxford almost coincided, as it happened, with the appearance of his two works, *Geology of Oxford* and *Oxford Stone*. A considerable variety of building stones, mostly Jurassic limestones from the surrounding country, had been used in Oxford over the centuries. Ten years previously (1937) Arkell and L. H. Dudley Buxton had been consulted on the best sources of material for the renovation of a number of colleges and had submitted a private report on the matter. The second of the works just mentioned was an outcome of the interest so awakened, and is an informed and well illustrated account of the history of the use of various stones for Oxford buildings, with notes on their weathering properties. The preparation of this work involved much research among original documents in college records. Arkell was also greatly interested in place-names and their relation to local geology and topography, and several papers from his pen dealt with names in Wiltshire, Oxfordshire and Dorset. The dialect terms of quarrymen also attracted his attention, and, following a paper (1944) on the local names for the stone beds in Purbeck and Portland, he set about systematically recording all terms of this nature that came to his notice, with a view to their eventual publication in a glossary. Later, it was found that Dr S. I. Tomkeieff, of King's College, Newcastle on Tyne, had been making a similar compilation in the north of England. Their forces were, therefore, combined, and an interesting work, *English rock terms, chiefly as used by miners and quarrymen*, appeared in 1953 under their joint authorship.

Publication of Arkell's monograph on the ammonites of the English Corallian beds by the Palaeontographical Society was completed in 1948, and the first instalment of a corresponding work on Bathonian ammonites appeared in 1951. Now established as the foremost living authority on Middle and Upper Jurassic ammonites, Arkell, in his Cambridge days, was increasingly inundated with specimens from all over the world for examination and report, the oil companies contributing their full share to this mass of material. In December 1950, and January 1951, he attended a congress in Egypt at the invitation of the Egyptian Government, and took the opportunity to examine the Middle Jurassic rocks of the Gulf of Suez, and then to

visit the Wadi Araba to search (unsuccessfully) for an exposure of Lower Liassic rocks that Figari Bey, in 1864, claimed to have found there. From Egypt he proceeded to Saudi Arabia and visited the extensive outcrops of Jurassic formations at Jebel Tuwaiq, first discovered by Philby. These rocks had yielded to geologists of the Arabian American Oil Company (under whose guidance Arkell visited the area) a series of ammonites which he described in 1952 in the *Philosophical Transactions*. In January 1952, a further opportunity for travel arose. Accepting an invitation from the University of Algiers, he visited Algeria and Tunisia and examined exposures of Jurassic rocks under the guidance of Professor H. Laffitte and Monsieur Gabriel Lucas. His last oversea journey was in September 1953, when, as guest of the German Geological Society, he visited the classic Jurassic exposures of Suabia.

The researches of these post-war years culminated in the publication of a further major work, the *Jurassic Geology of the World*, which appeared in 1956. Of about the same length and of the same format as *The Jurassic System in Great Britain*, this consists primarily of an able and critical digest of a very extensive literature, much of it bewildering to the average worker because of the number and varying usages of stratigraphical terms, but it also contains much previously unpublished information. The emphasis throughout is on ammonite palaeontology. From the time when S. S. Buckman promulgated his schemes of 'polyhemeral' chronology much controversy had centred upon the questions of the universality of ammonite zones and the degree of precision with which Jurassic time could be subdivided on the basis of the ranges of ammonites. In the short introduction to *The Jurassic Geology of the World* we find its author's mature judgement on these matters. 'Minute subdivisions,' we read, 'such as those made by Buckman in the English Lias, are not recognizable even in other European countries', and 'for correlations from one part of Europe to another the smallest units of practical value are astonishingly near the first set of zones promulgated by Oppel'; further, 'for the Upper Jurassic even Oppel's zones are often too small to be recognized outside Europe'. Similarly, most of the chronological-stratigraphical terms introduced for parts of the Jurassic succession by one author or another are considered unnecessary; in fact, for classification on a world scale, the original scheme of stages introduced by d'Orbigny in 1850 is considered preferable to any later one. Thus in this work the Jurassic System is divided only into d'Orbigny's original 11 stages and no more than 58 ammonite zones are recognized in the Jurassic of north-western Europe. The appearance of this work was followed in 1957 by that of one more or less complementary to it, the volume on Ammonoidea of the *Treatise on Invertebrate Paleontology*, edited by R. C. Moore. Arkell's contributions here consist mainly of the general introduction on Mesozoic Ammonoidea, of the systematic descriptions of most taxa of Jurassic age (although such descriptions are integrated with those of B. Kummel and C. W. Wright, who are primarily concerned with Triassic and Cretaceous taxa respectively), and of the

section on aptychi. It is probable that the classification and nomenclature of this exhaustive work will long be accepted as the standard ones.

In the early autumn of 1956 Arkell had a severe stroke which left him partially paralyzed. He bore this sad affliction with the greatest courage and by sheer will-power contrived after a time to resume some of his work and correspondence. The present writer, to his great regret, did not have the opportunity of visiting him at his home in Cambridge during this closing phase of his life, but those who did see him have commented on his cheerfulness and mental alertness, and on his unabated interest in events in the world of geology. He died within a few hours of a second stroke on 18 April 1958. His geological library, almost as complete as that of the British Museum (Natural History) in world Jurassic literature, has been bequeathed to the University of Oxford, in the anticipation that the Department of Geology there will become a centre for work on Jurassic palaeontology. His earlier fossil collections are at Oxford, the later ones mainly in the Sedgwick Museum, Cambridge. His wife and three sons survive him.

The National Academy of Sciences of America awarded Arkell the Mary Clark Thompson Gold Medal in 1944, the Geological Society of London the Lyell Medal in 1949, and the German Geological Society the von Buch Medal in 1953. He was an honorary member or correspondent of the Linnean Society of Normandy, of the Geological Societies of France, Germany and Egypt, of the Paleontological Society of America, and of the Dorset Natural History and Archaeological Society. He served on the Councils of the Geological and Palaeontographical Societies, and on several Royal Society committees.

Arkell was tall, fair-haired and fresh-complexioned. His robust appearance in the prime of life suggested that he could well have been an outstanding oarsman or athlete in his student days, but it was deceptive, for much of his life was a struggle against an inherently weak constitution. His manner was somewhat reserved and he disliked social functions, to which he found the solitude of the open country or the sea cliffs infinitely preferable. He was always, however, most approachable and friendly, and gave unstinted help and encouragement to younger workers and to amateurs. That he could be lighthearted as well as serious is shown by the 'field meeting report' entitled 'The London excursion, 1725', which appeared anonymously (but in fact from his pen) in the *Proceedings of the Geologists' Association* for 1946, and illustrates, incidentally, his interest in the history of geology. His pen was fluent and his prose style clear and unimbellished. He had firm convictions and was not slow to express his opinions on many subjects in unequivocal language; but it was never his intention to give offence, for he bore no ill-will towards those who disagreed with him. He preferred facts to theories; philosophical speculations entered very little into his writings and had no influence on his conclusions.

L. R. Cox

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R. M. Bailey

RICHARD WILLIAM BAILEY

1885-1957

RICHARD WILLIAM BAILEY was born at Romford, Essex, on 6 January 1885. His father, James William Bailey, was an official of the Great Eastern Railway; his mother was of farming stock. He was the second child, and oldest son, in a family of seven, of whom two died in infancy. There is no record of any of his kinsfolk being concerned with scientific or engineering work.

About the beginning of the century R. W. Bailey commenced an apprenticeship at the Great Eastern Railway Company's Locomotive Works, Stratford. While there he gained both a Whitworth Exhibition and a Whitworth Scholarship—in each case the first to be awarded to an apprentice from the Stratford Works. These awards were supplemented by a 'Director's Scholarship' from his Company; it is clear that even as an apprentice he was highly regarded. The scholarships enabled him to spend two winter sessions at East London Technical College, now Queen Mary College.

In the final year of his apprenticeship with the Great Eastern Railway (about 1906) the directors and officials of the Railway were considering improved means of coping with the heavy suburban traffic into Liverpool Street Station. One school of thought favoured electrification. The Chief Mechanical Engineer of the railway reacted to this by designing the 'Decapod', a steam locomotive with five coupled axles intended to give high acceleration in suburban service. As one of the few technically trained engineers on the railway staff, young R. W. Bailey was brought into contact both with the electrification proposals and with the 'Decapod' design.

The prospects of electrification were considered sufficiently bright to justify Bailey in spending a year on an electrical engineering course at the City and Guilds Central Technical College, South Kensington, followed by a year's college apprenticeship with the British Westinghouse Electric and Manufacturing Company. He came to these works about the time when apprenticeship courses were centralized under A. P. M. Fleming (now Sir Arthur Fleming) and a real effort was made to give systematic training to professional engineers.

The year 1908, when Bailey completed his course, was a time of business depression. The British Westinghouse Company was in financial difficulties and was unable to offer staff appointments to college apprentices coming out of their time. The Great Eastern Railway set aside their plans for electrification. Bailey spent the next four years as a lecturer in mechanical engineering

at the Battersea Polytechnic, London. He married, in 1909, Mary Florence Dormer Alderman, who survives him.

In 1912 Bailey was appointed by the Cheshire County Education Authority to be first Principal of the Crewe Technical Institute. This establishment was successor to the Crewe Mechanics' Institute which had been run by the London & North Western Railway Company to offer elementary technical education to their employees. Bailey was provided with extemporized buildings and a grant of £2000 for equipping an engineering laboratory, and had to build up a technical college from scratch. He gave great care to his students, both in their education and in guiding them to suitable employment; this was evident also in his dealings with the training of disabled soldiers and sailors during the war of 1914-1918. His concern that technical training should not be wasted is as clear in his first published work as in his Presidential Address to the Institution of Mechanical Engineers forty-one years later.

In June 1919 Bailey accepted an invitation from A. P. M. Fleming to return to the British Westinghouse Company as a senior member of the Research Staff. The Company had decided in 1910 to set up a Research Department, but it was only in 1917 that this Research Department was established under Fleming as Manager. It was Fleming's intention that the objectives of the Research Department should include fundamental research, applied research and raw material control. In Bailey he found an assistant eminently qualified to follow all these objectives and Bailey was placed in charge of the mechanical, metallurgical, and chemical sections of the Research Department.

Bailey remained in this Research Department for the remainder of his career. Shortly after his return the Company's name was changed to Metropolitan-Vickers Electrical Company Ltd.

Bailey quickly developed the small existing chemical laboratory and established separate mechanical and metallurgical laboratories. A research administration building was opened in 1920 and the Company's first laboratory building designed for its purpose was opened in 1921. This building included provision for the mechanical, metallurgical, and chemical sections, and probably constituted the country's leading industrial research department at that time.

Bailey organized systematic testing and inspection schemes for the material used in machines of the Company's manufacture, and extended these services as required both to material suppliers' works and to the Company's manufacturing departments. He developed the research testing of materials to provide new data, and set up an advisory service for the Company's engineering and manufacturing departments.

Bailey regarded his primary duty as service to the Company's engineering departments. His associations were particularly close with K. Baumann, the Company's gifted Chief Mechanical Engineer, and with H. L. Guy (later Sir Henry Guy, F.R.S.) the forceful Chief Engineer of the Mechanical

Department. During the years to 1950 Baumann was responsible for great advances in the design and rating of steam turbines, with increasingly onerous steam conditions and with continually enhanced thermal efficiency. These advances produced a series of material problems; larger forgings and castings with better properties were required, and there was continual search for improvements in resistance of materials to high temperatures. Bailey did work of the utmost importance in these developments in investigating the high temperature properties of materials, in developing improved materials and in discriminating between the suitability of various materials for engineering service. His work in high-temperature creep of metals is his outstanding contribution to knowledge.

The steam temperature which can be used in power plant is subject to limitation from conditions in the superheater and in the steam piping and flanges as well as in the steam turbine itself. Bailey devoted attention to problems affecting all these components, and in the case of steam piping and flanges he made noteworthy contributions to design as well as to material problems.

Bailey took continued interest in the prospects and progress of the gas turbine. In the 1920's he surveyed various thermal cycles and came to the conclusion that gas turbines had potential advantages in compactness and in flexibility of characteristics, but that straight gas turbine plant could then offer only poor fuel consumption due to temperature limitation with available materials. He commenced a systematic study of combinations of reciprocating engine and gas turbine plant, using the reciprocating engine for the high temperature portion of the cycle. At a later date he extended these studies to cycles which incorporated also steam turbine plant. He examined the prospects of the application of such cycles to marine propulsion; his work in this field is summarized in his *Andrew Laing Lecture* of 1949.

The advent of aero gas turbine development in 1938 presented extensive opportunities for the application of Bailey's knowledge of the behaviour of materials at high temperature. This knowledge was fully applied in war-time jet engine development for the service of all British aero gas turbine manufacturers.

In 1945 Bailey relinquished executive duties in the Metropolitan-Vickers Research Department and was appointed Consulting Engineer. In this capacity he was able to give fuller attention to the research and development work in which he was particularly interested.

Bailey played a prominent part in the work of many technical institutions and committees. He was elected F.R.S. in 1949 and served on the Council in 1951 to 1952.

His service to the Institution of Mechanical Engineers is particularly memorable. He was Chairman of the North-Western Branch in 1941, Member of Council from 1942, and Chairman of the Publications and Library Committee from 1949 to 1953. He was elected Vice-President in 1949 and President in 1954.

The 2nd of February 1954 was a critical day in Bailey's life. The weather was bitterly cold and he had been in poor health. He had a meeting in London which he considered it most important to attend, and he made the return journey from Manchester by train that day. On the way back he found that one leg was paralyzed. His induction as President of the Institution of Mechanical Engineers, to which he had so much looked forward, was due on 19 March. This induction was duly performed; it is stated that at the beginning of the ceremony he looked as if he could not carry through with it, but once in the Chair he appeared to revive.

In the arduous year of office as President which followed he was under grave physical handicaps, but was ably supported by his wife. That year included the Institution's Summer Meeting in Holland.

Bailey continued to attend his office, and to play his part in investigating material problems, but with increasing difficulty. His formal retirement from service with Metropolitan-Vickers took place on 1 April 1957. At speeches accompanying the presentation he was much affected.

Bailey had no hobbies, and after his retirement he continued to work at home. On 15 June he was taken by his older daughter to visit his younger daughter at Hinckley, Leicestershire. He could scarcely walk at this stage and in spite of every attention from his daughter and her husband, both physicians, he gradually sank. He died on 4 September 1957.

SCIENTIFIC WORK

1. *Creep*

Creep of metals is the subject with which R. W. Bailey's name is especially associated. The importance of creep, that is the slow deformation of a material with time under load, was first appreciated in mechanical engineering in the early 1920's. For the structural materials then in use, creep was of no importance at temperatures below 350 °C, and so many different troubles afflict plant in service at elevated temperatures that creep was slow to receive separate recognition. A publication by J. H. S. Dickenson in 1922 was noteworthy in that he showed the inadequacy of short-time tests.

The significance of creep in steam turbine operation was recognized at Metropolitan-Vickers and in 1924 a programme of experimental work on creep was put in hand under Bailey's supervision. Bailey realized that it was necessary to have test equipment of really high sensitivity capable of operating for long periods, and equipment to satisfy these requirements was developed by his assistant, A. M. Roberts.

Bailey realized also the magnitude of the experimental programme which would be required to obtain adequate long-life information on the creep of engineering materials, and in letters to the editor of *Engineering* in 1925 he appealed for co-operation and concerted effort by workers in this field. One outcome of this correspondence was the formation of a committee of the British Electrical and Allied Industries Research Association to which Bailey

devoted great service. For many years however the creep investigations in Bailey's own laboratory were of greater fundamental significance than those of any other establishment.

Accurate creep strain information soon became available, and this flow of creep data has continued. An early result of the work was to explode the concept of 'limiting creep stress' which was widely held at the time. It was found that as the sensitivity of measuring equipment was increased, creep was detected over a continually widening range of conditions, confirming Bailey's early conviction that there was no limiting stress for a particular material and temperature below which creep suddenly stopped.

From the first, attention was directed by Bailey to creep rate rather than to any 'limiting creep stress'. It was established that in the usual creep curve under constant stress and temperature, there were three stages of creep—the primary stage, a period of diminishing creep rate; the secondary stage, where creep rate varied little with time; and the tertiary stage, a period of increasing creep rate ending in rupture. In engineering practice it is necessary both to keep within permissible permanent deformation and to have an adequate margin against rupture.

In steam turbine design Baumann quickly took account of the creep information supplied by Bailey in designing components exposed to high temperature. In a steam turbine which has a service life of 100 000 hours, the deformation permissible in a shrunk-on disk may be taken as 0.01 per cent in the whole working life; this implies a mean creep rate not exceeding 10^{-9} strain per hour. Information for 100 000 hours' life was not available from creep tests, and for a long time there were hardly any test results of as long as 10 000 hours. The assessment of long life creep strain therefore required extrapolation over a wide range. After considerable investigation, Bailey came to the conclusion that extrapolation on a basis of temperature is better than extrapolation on a basis of stress; he presented his data in the way which would be most useful for this purpose, and gave cautious assessments of long-life conditions which proved more satisfactory than forecasts by some other methods.

Bailey early realized that creep was not an isolated phenomenon and that it must be studied in conjunction with other changes in the structure and properties of materials. In 1929 he reported that metallurgical changes characterized by spheroidization of the pearlitic carbides in steel can occur at temperatures as low as 350 °C. In 1932, with A. M. Roberts, he showed that this change in structure could be accompanied by a marked reduction in creep strength, and reported that the rate of change with temperature for effects of this type obeyed an exponential law.

The results of this work included the development of improved materials of construction. Nickel-chromium-molybdenum steel was already in regular use as a high tensile material, and it was put into service in turbine construction in some instances as a replacement for other steels which had given trouble by creep. Bailey found that the nickel content of this material was

not advantageous in creep, and he was responsible for the first chromium-molybdenum steels used for bolts in high-temperature service. He found that 0.5 per cent molybdenum steel had superior creep properties and went on to investigate the effect of adding other alloying elements to this molybdenum steel. The major outcome was the discovery of the remarkable creep properties of molybdenum-vanadium steel, which has been used on a large scale for steam turbine and gas turbine construction all over the world and which with minor modifications remains the best ferritic steel for long-life service at about 550 °C.

In different steam turbine components different deformations are permissible during a service life of say 100 000 hours; Baumann in 1930 published permissible figures for various components. In some steam turbine components, creep conditions are so complicated that only empirical methods of design can be used. Steam turbine diaphragms provide a case in point. The deflexion of a diaphragm in service is the sum of elastic deflexion and permanent set, large deflexion can be associated with moderate local strain, stress and strain conditions are extremely complicated, and load and temperature vary with turbine operating conditions. In the early 1930's the present writer, then on H. L. Guy's staff, spent a good deal of time measuring the permanent set of diaphragms on steam turbines while on overhaul, and endeavouring to correlate the loading conditions on these diaphragms with service records from station log sheets. Bailey's advice was freely available in reducing this chaotic set of creep information to simple rules for diaphragm design.

Bailey's 1935 paper to the Institution of Mechanical Engineers is a landmark in the knowledge of creep. In it he set out to provide a rational basis for the design of parts required to operate under creep conditions and under any system of stress. He had previously done much work on creep under compound stresses, including work on lead tubes at atmospheric temperature which were found to exhibit similar behaviour to steel tubes at elevated temperature. He had established that creep depends on the shear stresses and that it is not a function of the maximum shear stress alone. In his 1935 paper he suggested a general relationship determining creep in the direction of the three principal stresses. He investigated stress redistribution as a consequence of creep in various engineering parts under various types of load. He dealt extensively with creep under diminishing stress as a strained system relieved itself (now called creep relaxation). This condition is of special interest in pipe flanges and bolts.

2. Steam piping and pipe flanges

Bailey devoted great attention throughout his working life to the design of steam piping and pipe flanges for the continually more exacting conditions in power stations. This afforded ample opportunity for application of his knowledge of properties of materials in conditions of creep, embrittlement and corrosion, and also for his ingenuity in design. His services were much in

demand as a consultant on power station piping problems, by customers as well as by his own Company, and users frequently collaborated with him in service trials in power stations.

He made many proposals for flange designs to give improved service and a number of his patents are in this field. His endeavours to keep flange design on a rational basis and adequate to meet current demands led to major contributions to the work of the Pipe Flanges Research Committee of the Institution of Mechanical Engineers and of the Pipe Flanges Committee of the British Standards Institution.

Bailey's two papers to the Institution of Mechanical Engineers in 1951—the first intended for design engineers and the second for specialists—form an important contribution to knowledge in this field. There had been considerable advances in steam pressure and temperature in generating stations recently put into service, particularly in the United States. In Bailey's view much of the steam piping in these stations was unnecessarily extravagant both in material and in scantlings. He points out that for steam piping under high pressure and temperature conditions, creep behaviour will dominate performance in service, and that this has a number of consequences not anticipated in conventional design.

Bailey considers particularly the austenitic steel piping which had been extensively used in the United States, and queries whether it would show the same advantages over ferritic steel in 100 000 hours' life as are apparent in 1000 hour creep tests.

He makes a systematic examination of the effects on steam piping of the fluctuations in pressure and temperature which occur in service. The austenitic steel has some advantage in allowing higher temperature peaks when they total a small fraction of the whole life.

He discusses flange designs for high steam conditions, and makes proposals for a removable joint which could be used between austenitic piping and ferritic piping (which have different coefficients of thermal expansion). He considers also piping for less advanced conditions, and shows the possibility of economies as compared with practice then current.

3. Jet engine development

At the beginning of 1938 Metropolitan-Vickers started gas turbine development for aircraft propulsion under contract from the Air Ministry. The early work was done in close collaboration with the Royal Aircraft Establishment. Two members of the R.A.E. staff (A. A. Griffith and H. Constant, both later F.R.S.) originated the ideas which were reduced to hardware by Metropolitan-Vickers.

The use of high temperature is vital to the performance of any type of gas turbine. The development of new high temperature materials for gas turbines was carried out mainly by the material manufacturers, but Bailey's laboratory played an important part in checking material properties and

Bailey's advice was readily available both to the material manufacturers and to the engine designers. This advice was influenced by his preference for an established material when the claims for a novel material were incompletely proved, and by his suspicion of any material which owed apparently good creep properties to a structural change which might be transient.

The high temperature materials recommended by him for the first experimental gas turbine were austenitic steels— 'Rex 78' for the turbine, 'Immaculate V' for combustion chamber sheet metal, and 'F.D.P.' for combustion chamber flanges and fittings. The turbine material served its immediate purpose but was rendered obsolete for gas turbine blading in the course of a few years as the Nimonic materials (nickel-chrome base alloys) became available. The combustion chamber materials were commercial austenitic steels which remained standard for their purpose throughout the period of Metropolitan-Vickers aero engine development.

From mid-1940 this development was concentrated on straight jet engines. On the early jet engines alternative materials were tried for the turbine disk— Rex. 78, Nimonic 75 and Bailey's own Molybdenum-Vanadium ferritic steel. All three were successful and the choice for future jet engines naturally fell on 'Moly-Van'—the cheapest, the easiest to obtain and the easiest to work of the alternatives tried.

Bailey's laboratory was always available to assist with the special problems which arose in development. For some time there was a great deal of cracking of turbine fixed blading, subject to hot spots from the combustion chamber. Bailey devised a very simple piece of equipment for thermal shock testing, consisting of a rig in which the test piece was exposed in alternate minutes to air and to a gas flame. With this rig he tested a number of high temperature materials and the order in which he placed them was consistent with the order given by later experience for those materials which were run in jet engines.

With war-time conditions and the entry of established aero engine firms to the gas turbine field, Bailey's advice was sought by the Ministry of Aircraft Production and by a wider circle of manufacturers. Following the establishment of the Gas Turbine Collaboration Committee in November 1941, Bailey became a member of the Mechanical and Metallurgical Sub-Committees, and for several years played the leading part in the work of the Disk Panel of the G.T.C.C.

His analytical and experimental work on stressing and creep in jet engine disks under centrifugal loading and temperature gradient is covered in a series of reports to the Ministry of Aircraft Production.

Bailey's advice on jet engine materials had a remarkable consequence on the fortunes of the last jet engine to be developed by Metropolitan-Vickers—the Sapphire. This engine had 'Moly-Van' disks and Nimonic turbine blading. The Sapphire was taken over for later development and manufacture by Armstrong Siddeley Motors and was licenced by them to Curtiss-Wright in the United States. When an evaluation was made by the

United States Government of the strategic materials content of jet engines manufactured in the United States, the strategic materials content of the Sapphire came out a tiny fraction of the value for every other engine. There can be little doubt that this contributed to the fact that 8000 Sapphires were built in the United States.

4. Organization of research

Bailey's lifetime experience in the organization of research for industrial purposes is summed up in his 1954 Presidential Address to the Institution of Mechanical Engineers, 'Research in mechanical engineering'. He points out that what is described by the term research is very old in the best engineering practice. He states repeatedly that the technical quality of the leader and senior staff is of far more importance in a research organization than lavishness of buildings or equipment. He shows the importance of obtaining a substantial solution of a problem quickly rather than a more complete solution at a much later date, and discusses the allied question of the interval between discovery in research and application of the result. He emphasizes the importance of close contact between design and research staff, particularly in the matter of failures.

Bailey devotes a considerable portion of the Address to a comparison of research conducted by a firm on its own account with research conducted for a firm by an external agency, such as an industrial research association or a university. With his wide experience of both classes of research, he points out the conditions which favour choice of the one arrangement or the other, and shows how a firm may derive maximum benefit from either class of research. His statement that committee policy and decision may be less informed than its more expert members is a clear cry from the heart.

In these notes the writer has endeavoured to show that Bailey was a man who combined outstanding engineering and metallurgical skill with remarkable insight in the research field. He was fully capable of meeting both design engineers and steel manufacturers on their own ground. Bailey's capacity for work and fertility of mind is evidenced by the 88 British Patents in his name as well as by the bibliography of 36 published papers.

As an engineer in contact with Bailey for over thirty years, the writer can bear witness to his approachability and his charm of personal character. It was characteristic of Bailey's modesty that he left no personal record with the Royal Society. The writer is much indebted for information used in this memoir to Bailey's colleagues at Metropolitan-Vickers, particularly to Dr L. E. Benson. The writer would also express his deep gratitude to Mrs R. W. Bailey.

D. M. SMITH

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W. W. Benson.

WILLIAM NOEL BENSON

1885-1957

WILLIAM NOEL BENSON was born near London on 26 December 1885, the son of William Benson, an English Quaker, who was a shipping manager, and who shortly afterwards took up residence in Tasmania. Benson was educated at The Friends' School, Hobart, and later at the University of Hobart and the University of Sydney, where he came under the influence of the inspiring personality and teaching of Edgeworth David. While still at Sydney and before even graduating, he published a paper on the contact aureole of a granitic body. He graduated in 1907 with First Class Honours in Geology-Mineralogy, and in the following year, during Mawson's absence in Antarctica with the Shackleton expedition, he acted as Lecturer in Mineralogy and Petrology in the University of Adelaide.

Three papers followed this sojourn in South Australia, two on the petrology of Pre-Cambrian and Cambrian rocks from the Barossa Ranges and Blinman, in which he postulated the existence of a titanium-rich Houghton magma, and one on the geomorphology of the Mt Lofty Ranges, in which he was the first to put forward the idea of Cainozoic step-faulting.

He returned to Sydney and demonstrated in the Geology Department of that University from 1909 to 1911. Late in 1910 he won an 1851 Exhibition Science Scholarship and went to Cambridge in 1911, where he studied under Harker, Bonney and Marr. Before leaving N.S.W. (actually about 1909) he had started work on the Great Serpentine Belt along the margin of the New England Tableland. His primary interest there was in the serpentines and their tectonic environment, but he also became interested in the Devonian spilites, dolerites and keratophyres and he took a collection of these as well as of the ultrabasic rocks with him to Cambridge. At that time attention was being focused in England on spilites, especially through the work of Dewey and Flett, and Benson's discovery of one of the finest displays of them in the world—in the Great Serpentine Belt—was most timely. The petrological work he did on these and on the serpentines was published in the *Proceedings of the Linnean Society of N.S.W.* (Vol. **38** (1913), Vol. **40** (1915)).

At the conclusion of his two years' research at Cambridge he received the B.A. (research) degree (the Ph.D. degree was not then established at Cambridge). Most of 1913 was spent travelling in Europe with his father, mother and sisters, visiting areas of geological interest and examining petrological collections in universities and scientific institutes, as well as viewing places of scenic and historic interest. On his return to N.S.W., having been appointed to a Macleay Fellowship in Geology, he continued work on the Great

Serpentine Belt. During his absence abroad two parts of a series of papers on the Belt had been presented to the Linnean Society of N.S.W., one on the general geology, and the other on the geology of the Nundle district, some distance south of Tamworth. The first paper embraced the geology, including maps and sections of an area of about 1900 sq. miles stretching for over 100 miles from N. to S. along the western flank of New England. With no other means of locomotion than a 3-speed gear bicycle and his long legs he did the reconnaissance, mapping and examination of this in three months, and, although he inevitably made some errors of observation and interpretation, later workers have been amazed at the accuracy and insight displayed by Benson and the amount of detail observed by him. In these papers (9 in all, some written after his departure to New Zealand), he laid the foundation of our knowledge of the Devonian rocks of western New England and of the marine Lower Carboniferous, as well as adding materially to our knowledge of the Tertiary basic flows and intrusions and the geomorphology of the area. His New South Wales studies, combined with his wide acquaintance with other people's work, formed the basis of his subsequent general papers on 'The origin of serpentine' and 'The tectonic conditions accompanying the intrusion of ultrabasic rocks'.

It would seem that at first he thought the spilites, dolerites and serpentines of the Great Serpentine Belt to be elements in one great related suite and to be contemporaneous, but later it became clear that the spilites are Middle Devonian at the youngest, and that the serpentines are much later—Carboniferous, Benson thought, but more probably late Permian.

It had been his plan to re-examine and describe in greater detail the various parts of the Great Serpentine Belt and extend the work south from Tamworth, and two papers were prepared to this end, one on Tamworth (*Proc. Linn. Soc. N.S.W.* **40** (1915)) and one on Loomberah (*ibid.* **43** (1918)), filling the gap between Tamworth and Nundle, but his field work was interrupted in 1915, when he was seconded to help in lecturing work in the Geology Department of the University of Sydney, and was finally abandoned when he left for New Zealand in 1917. In the last investigation he left the Devonian rocks and the Serpentine line and made an offset to the west of Tamworth to study the Carboniferous succession in the Currabubula district. This was of interest for the discovery of the northerly extension of the Carboniferous glaciation recognized at Seaham in 1914. The Currabubula work was largely reconnaissance, but Benson's account was the only one available till the area was resurveyed and a drastically modified interpretation published by Carey in 1937.

Apart from his Serpentine Belt work in New South Wales, Benson published a number of papers, petrological mainly, on the Hornsby and Dundas volcanic necks near Sydney, on some of the interesting nepheline-bearing dolerites of the Liverpool and Mt Royal Ranges, and, while still a student, on the geology of the Newbridge area, near Orange.

He was appointed Professor of Geology at Otago University, Dunedin,

New Zealand, in 1917. Here he completed his series of papers on the Great Serpentine Belt and a group of studies arising therefrom, viz., 'The Devonian fossils of Australia,' and his memoirs on the 'Origin of serpentine' and 'Tectonic conditions accompanying the intrusion of the basic and ultrabasic igneous rocks'. A summary of the geology of New Zealand was published in 1921 and was followed by discussions of 'Palaeozoic and Mesozoic seas in Australasia' and the 'Structural features of the margin of Australasia' as traced from the East Indies to New Zealand. In connexion with Sir Charles Hercus's work on the incidence of goitre in New Zealand, a study was made of the sources of iodine in the soils and waters of that country. In association with R. A. Keble of Melbourne, the graptolite faunas and their sequence in the Ordovician rocks of New Zealand were studied, and he worked, in part in co-operation with the later Professor Bartrum, on plutonic and metamorphic rocks in south-western New Zealand, introducing modifications into then current views concerning post-Cretaceous earth movements and geomorphogeny in southern New Zealand. His chief research work in that country, however, has dealt with the nature and tectonic environment of Cainozoic volcanic rocks, and especially the varied features of the general geology, and the petrology and sequence of the igneous rocks in eastern Otago on which he has published a number of papers, several of which were written in association with Dr F. J. Turner. He retired from the staff of the Otago University in 1951 and was appointed Emeritus Professor of Geology. A tireless and energetic worker in field and study, his output and range of published research was amazingly large and though his geological outlook was wide he was a master of detail.

He was president of Section C (Geology) of A.A.A.S.—the Australasian Association for the Advancement of Science* in 1921 and of the Royal Society of New Zealand from 1945 to 1947. Other honours bestowed on him included the award of the Lyell Fund (in 1933), and the Lyell Medal (in 1939) by the Geological Society of London, the Hector (in 1933) and Hutton (in 1944) Medals of the Royal Society of New Zealand, the Clarke Medal of the Royal Society of N.S.W. (1945), the Mueller Medal of A.N.Z.A.A.S. (1951). In 1949 he was elected a Correspondent of the Geological Society of America and in 1951 a Fellow of the Royal Society. Later (1954) he was elected to honorary membership of the Mineralogical Society of London.

A genial soul with an eager and enthusiastic nature, Benson was a warm-hearted friend, and ever generous in his appraisal of his colleagues and their work. His abstraction through preoccupation with his work was proverbial, making him the perfect model of the 'absent-minded professor', and around his personality a garland of stories, some founded on fact, others apocryphal but good enough to be true, was woven. Benson was a very successful, inspiring and well-beloved teacher. Despite the initial handicaps at Dunedin

* Later A.N.Z.A.A.S.—the Australian and New Zealand Association for the Advancement of Science.

of entire lack of staff and poverty of equipment he succeeded in building up an efficient department of which many geologists filling important posts in New Zealand and abroad are proud to proclaim themselves graduates.

In the early days of his incumbency of the Otago Chair, conditions were anything but comfortable. Apart from inadequate housing the department suffered from lack of staff and equipment. Benson had to lecture and demonstrate on all branches of the geology course without help, and even had to put out and put away his lecture-specimens and clean blackboards.

He was irked by isolation in his new environment, there being no other colleague of similar geological interests with whom to talk.

Though much wrapped up in his New Zealand work he never lost his interest in Australian, and particularly in N.S.W. geology. He retained his membership of the Linnean Society of N.S.W., and apparently read the geological papers therein. In his letters he often showed himself generous in praise of the work of other geologists.

He was married on 8 December 1923 to Gertrude Helen Rawson who survives him. There were no children except twins who were stillborn. His wife had been a lecturer in the Otago University Department of Domestic Science and before her marriage had succeeded to the Chair.

Owing to a breakdown in health he retired from active teaching in the University a year before he was normally due to retire, but retained contact with the Department and with research. His health apparently improved and he was able to take an overseas trip with his wife in 1954-5, during which he worked on his Otago volcanics and on the recently discovered Middle Cambrian fossiliferous rocks of the South Island.

At the time of his death he had in hand a revision of his graptolite bibliography, the entries in which he had kept up to date since its first publication in 1938, and was also writing an account of the physiography of Brisbane Water, N.S.W., in addition to finishing off his Dunedin petrology.

The importance of the role a man of Benson's type plays in the intellectual development of a region as large and as culturally youthful as Australasia can be estimated only with difficulty. Inspired by his penetrating yet broadly sweeping approach to geology, his students and colleagues may lift the local level of attainment in their science so that the work of the pioneer is rapidly dated. Very often the able man drifts into administration in his later years, but not so Benson. He remained for ever a student, with all a student's enthusiasm for the new discovery and the highest levels of technical skills. Benson's greatest memorial derives from his work, but lies more in its impact on the lives of his students and his successors than in the scientific weight of the work itself, first class as this undoubtedly was. To have performed what he did at the times and places he worked in is itself an indication of his profound geological intuition. His understanding of the earth has been implanted in his writings and his teaching, and its fruits are to be seen in our growing comprehension of the geology and tectonics of Australia and New Zealand.

In the preparation of this biographical memoir, I have made extensive use of information and script very kindly supplied by Dr W. R. Browne of Sydney and of several publications about Benson dating from 1951.

E. S. HILLS

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S. L. Chapman

DAVID LEONARD CHAPMAN

1869-1958

DAVID LEONARD CHAPMAN was born in Wells, Norfolk on 6 December 1869, and died on 17 January 1958. His father, David, was a none-too-successful builder in Manchester; his mother, Maria Wells, came from a family with a well-established malting business in Wells, and her mother was a friend of Lady Nelson. She possessed a strong Victorian character, and was determined that David, the eldest son, and his brother Sydney (afterwards Sir Sydney Chapman, Professor of Political Economy at the University of Manchester) should have opportunities of education. She sent them first to an old-fashioned dame's school and afterwards to Manchester Grammar School. Here he was taught by Francis Jones, and in writing his obituary in 1926 Chapman states 'Jones started his work at the Manchester Grammar School in 1872, during the period of a great scientific awakening when popular science lectures attracted large audiences in the chief industrial centres. Nevertheless, science was considered to be rather vulgar by many of the educated classes. It was fourteen years later that the writer, in opposition to the wishes of the High Master, entered the Science V, the refuge of the incorrigibly stupid. The High Master in giving his consent uttered the warning that his (the writer's) chance of becoming a well-educated man would thereby be lost.' In due course Chapman won an Open Exhibition at Christ Church, Oxford, and was placed in the First Class of the Final Honour School of Natural Science (Chemistry) in 1893. In the following year he was awarded a Second Class for Physics in the same Final School. He then took a post as science master at Giggleswick, but was soon picked by Professor H. B. Dixon to be a member of the staff at the University of Manchester, where he spent the next ten years. In 1907 he accepted a Fellowship at Jesus College, Oxford, with the responsibility of arranging the equipment of the new College laboratories named after Sir Leoline Jenkins. He remained as College tutor with charge of the laboratories until his retirement in 1944.

Before the First World War Chapman visited Germany a number of times during University vacations, having learnt his German at 'Tilly's'. His outdoor interests were golf (for which he once won a medal at Settle Golf Club), cycling and walking. He sometimes cycled one hundred miles in a day, and stories are told of unfortunate misunderstandings when, as Estates Bursar, he visited College tenant farmers in the Yorkshire dales by this mode of transport. At the age of seventy-four he ascended Skiddaw with his wife, and remained physically strong and active until he was eighty-two. Painting in water-colour was a hobby he occasionally practised. He had received a very

sound mathematical training early in life, and always emphasized the great importance of mathematics for the work of the chemist as well as for that of the physicist. When the theories of relativity and of wave-mechanics were first put forward he studied them with great interest but found it difficult to accept their philosophical implications. He therefore felt it important to examine and criticize their foundations. After retirement he took up the study of philosophy, his preference on the whole being for Kant, lightened by an almost equal devotion to the works of Agatha Christie.

In manner Chapman appeared reserved and austere; his lectures, given with a penetrating and somewhat spasmodic voice, seemed dry and abstract at the time of delivery, but students found on re-reading their notes after later experience how completely and yet economically the subjects had been covered. His pupils were always devoted to him, and he was invariably helpful to anyone who sought his advice. He was extremely kind-hearted underneath an undemonstrative exterior. Without asking for College assistance he paid out of his own pocket the fees to the Technical School for the training of his laboratory staff, in consequence of which some of them were afterwards able to take very responsible technical posts. In scientific conversation he was very quick in seizing the essential elements in a problem, and ready to pose unexpected questions which astonished because of their penetration. One of his colleagues has referred to 'his gift for winning affection and respect, owing something to his gentleness of manner and to his artless, unaffected nature'.

In 1899 Chapman published his first paper. It was entitled 'The rate of explosion in gases' and presented the first sound theoretical treatment of what is now called detonation, though that word does not appear in the paper. Professor H. B. Dixon had delivered the Bakerian Lecture of 1893 on his observations on the velocities of explosion in gases. It was the values of these detonation velocities that Chapman succeeded in explaining most successfully. Previous attempts to explain them had been made on the basis of Dixon's hypothesis that 'an explosion is in its character essentially similar to a sound wave'. This suggestion had also been made by Berthelot.

Chapman, following Professor Schuster's ideas, which the latter had proposed in a note to Dixon's Bakerian Lecture, supposed that on either side of the 'explosive wave' in which reaction took place, are 'the exploded and unexploded gases which are assumed to have uniform densities and velocities'. To this situation Chapman then applied Riemann's equation relating the gas movement to the pressures and densities of the gases in front of and behind the wave. However before the *uniform* explosion velocity could be calculated, it was necessary to make an assumption. This assumption, which has been entirely justified by subsequent work, was argued by Chapman in the following words:

'When an explosion starts its character and velocity are continually changing until it becomes a wave permanent in type and of uniform velocity. I think it is reasonable to assume that this wave—i.e. the wave of which the

velocity has been measured by Professor Dixon—is that steady wave which possesses minimum velocity; for, once it has become a permanent wave with uniform velocity, no reason can be discovered for its changing to another permanent wave having a greater uniform velocity and a greater maximum pressure.’

Later in the paper he pointed out that ‘the condition of minimum velocity is equivalent to the condition of maximum entropy’. Chapman therefore made two contributions of major importance to the theory of detonation. The first was the application of Riemann’s formula and the second, the assumption that the observed velocity is the minimum velocity that is consistent with the other conditions.

Before Chapman could calculate the explosion velocities, however, he had to be able to determine the thermodynamic condition of the exploded gas using the heat evolved in the chemical reaction. Since the final temperatures were often above 3000 °C this was not straightforward. What he did was to use some of Dixon’s measured velocities to calculate the specific heats of the product gases and then, employing these, he calculated the explosion velocities of some forty other mixtures with great success. As regards the specific heats, he concluded that those of ‘O₂, H₂, N₂ and CO might for all practical purposes be taken as identical at all temperatures’ and that ‘the specific heat of steam rises more rapidly with the temperature than the specific heat of the diatomic gas’. He provisionally discarded the alternative view that steam dissociated to any great extent over the range covered by Dixon’s experiments (up to 4300 °C).

Chapman’s great and lasting contributions to the theory of detonation, which were later independently made by Jouguet, have been commemorated by the state of the exploded gas immediately behind the explosion wave being called the ‘Chapman–Jouguet state’.

Chapman’s attitude towards research remained always unhurried and un-self-seeking; he believed in strict accuracy combined with caution in stating precisely what had and what had not been proved. He clearly followed the existing literature, and was quick in spotting weaknesses in earlier work. Many of the experimental matters he examined were taken up because the supposed facts seemed inadequate or incorrect. In very early work he showed by vapour pressure and other measurements that ‘metallic’ and red phosphorus were identical, that the vapour molecule was P₄, and that the so-called phosphorus suboxide was really impure red phosphorus. He also gave a very clear theoretical picture of the stability relationships of the yellow and red forms of phosphorus. Some well-arranged experiments on the decomposition of water vapour by the electric spark disposed of a theory that it could be explained by the same concepts as those for liquid electrolysis; hydrogen was found at both electrodes, and that from the cathode sometimes considerably exceeded the amount collected in a voltmeter connected in series. He examined the iodine-tri-iodide system by electrical conductivity and voltmeter methods and was able to confirm the presence

of the I_3^- ion. In another investigation he studied the reaction between acetylene and mercuric chloride solution, in which a reactive addition compound is formed.

In three theoretical papers about the time of the First World War he discussed the subjects of electrocapillarity, the calculation of latent heats of volatilization from the volume relations of liquids and their vapours, and the derivation of a new equation of state.

The *Annual Reports of the Chemical Society* for 1914 contain an article by Chapman on 'General and physical chemistry'. The Bohr theory of atomic spectra (1913-14) is there fully described. In discussing the importance to chemists of the quantum theory he refers to Planck's law as follows: 'Stated in a simple form, the law is this: particles of matter emit and absorb energy not slowly and continuously, but in "jerks". In other words, the process of emission of energy is assumed in all cases to be analogous to the process which occurs when a molecule changes into an isomeric form. The latter process has always been assumed by chemists to be a sudden one, and therefore accompanied by a sudden evolution of energy. Planck's hypothesis, therefore, is equivalent to the assertion that all energy changes in matter are of the same character as those which occur in chemical change. The discontinuous character of all chemical change has become so familiar to chemists that it has ceased to be regarded as strange, or as needing explanation. Yet Planck's generalization is considered by some physicists to involve the abandonment of the principle—*Natura non facit saltum*—and the denial of the continuity of dynamic effect.' He further showed in a footnote that the expression for the distribution of energy in the spectrum of a total radiator can be much more elegantly deduced from the van't Hoff isochore than by Planck's original method.

Fifty years ago no homogeneous gaseous reaction was recognized as such, and Chapman was the first to venture into this field. His remarks on the subject show the clear and direct approach which was so characteristic of his work. 'Judging by the published experience of those chemists who have made a special study of chemical changes in gases in recent years, and particularly since Victor Meyer investigated the conditions of the union of hydrogen and oxygen, we should doubtless be compelled to conclude that slow pyrogenic changes in gases have their seat of action principally, if not entirely, on the walls of the enclosing vessels, and that any attempt to measure the velocity of change within a gas itself would be unsuccessful, owing to the impossibility of estimating the precise effect of surface. A moment's consideration, however, will make it clear that if we could be certain that only a small fraction of the total chemical change were confined to the gas not in the neighbourhood of the walls of the containing vessel, then the effect of surface might be almost entirely eliminated by increasing the size of the vessel until the ratio of its internal surface to its volume became very small. The only difficulty in the way of measuring the velocity of a homogeneous chemical change in a gas would then be the practical one attending the use of a vessel the dimensions

of which were large enough for the purpose.' Applying this logical conclusion to the change $\text{ozone} \rightarrow \text{oxygen}$ he was able to demonstrate the homogeneous nature of the reaction at 100 °C. The reaction proved to be nearly of the second order, and was not inhibited by intensive drying, but the effect of temperature was not examined.

The subject which absorbed Chapman's research energies for the longest period of time was the photochemical combination of hydrogen and chlorine. It is now-a-days very difficult to appreciate the pre-quantum concepts used by early photochemists. The reaction was first observed by W. Cruickshank in 1801, and first quantitatively examined by J. Draper in 1845; then again by R. Bunsen and H. E. Roscoe twelve years later. In 1905 when Chapman took up the problem Draper's view that 'the agency which effects the change is tithonicity, an essential constituent of sunlight' still appeared a useful hypothesis. Until the First World War the importance of the quantum theory as a basis for the interpretation of photochemical reactions was not realized; nor, of course, was any notion of a chain reaction in use. In the hydrogen-chlorine reaction there is an enormous contrast between the feeble light energy needed and the large energy of reaction liberated, and this appeared to make atom formation inconceivable. The effect of light was vaguely attributed either to 'a gradual breakdown of a resistance which opposed the force of affinity' or to an increase in the electro-negative properties of chlorine, which thereby 'exalts the affinity between chlorine and hydrogen'.

We now regard the hydrogen-chlorine reaction as one with an essentially simple mechanism. Chlorine molecules, on absorbing a light quantum, dissociate into atoms; these react with hydrogen molecules to give hydrogen chloride and hydrogen atoms, and the latter attack further chlorine molecules to renew the supply of chlorine atoms (W. Nernst 1918). A self-repeating reaction chain is thus set up to give a large number of product molecules per quantum of light absorbed. It is characteristic of chain reactions that the changes of rate with experimental variables such as reactant concentrations, other substances present in the system, walls of the vessel, etc., depend very critically on the 'chain-ending' processes operative. Under conditions of complete purity of the reactants, impossible to obtain in practice, the reaction chains would be terminated by mutual interaction of atoms or by catalytic wall effects. In fact the observed rates were found to vary in a capricious matter and many years of controversy and disagreement were consumed in securing reliable facts. Never has a reaction been so bedevilled by erroneous observations and incorrect conclusions, and such an amount of labour devoted to the correction of earlier reported results. This was the field where Chapman and his co-workers, including his wife Muriel Chapman, worked steadily, cautiously, and authoritatively to put the subject on a really firm foundation.

The first success (Chapman & Burgess 1906) came with the solution of the mysterious problems of 'induction' and 'deduction'. In order to follow the

change early workers confined the gases in a vessel containing a little water to absorb the hydrogen chloride formed and so to give a measurable volume change. It had been found that the reaction did not begin immediately the apparatus was exposed to the light; nothing appeared to happen for a time called the 'induction period', after which the rate rapidly rose to a constant value. Repeated experiments with darkening and illuminating the apparatus caused the induction period to shorten, but after a long period of darkness it was found to have regained a good deal of its former length. Earlier workers had also attempted to discover whether previous illumination of the chlorine by itself had any effect on the induction period after the addition of the hydrogen and had obtained conflicting results. Chapman and C. H. Burgess showed that such pre-illumination never led to a period of 'deduction', that is, an induced initial extra activity of the chlorine when mixed with the hydrogen, followed by a fall in the rate to the normal. By means of very careful experimentation he was able to show that induction periods were caused by traces of ammonia in the water used in the apparatus, together with other nitrogenous matters more slowly reactive with chlorine which were responsible for delayed effects. He found that as little as one molecule of NCl_3 in a million molecules of mixed gases reduced the photochemical sensitivity one hundred times. The way was immediately open for the development of a high-temperature chlorination technique for the apparatus to decompose nitrogenous matter and to eliminate the exasperating irreproducibility due to its presence. A further matter disposed of by Chapman and Burgess was the alleged existence of 'photochemical extinction', i.e. that a mixture of hydrogen and chlorine absorbs more light, owing in some way to the photochemical change, than a mixture of chlorine with air. With P. S. MacMahon he was able to disprove Bunsen & Roscoe's statement that slight variations of composition of the mixed gases greatly affected the rate, and to begin a study of the inhibiting effect of oxygen, later extending the work to other inhibitors. By 1909 Chapman had reached a firmly based stage of being able to claim that 'the light absorbed by the chlorine is not immediately degraded into heat, but is, as it were, stored up as efficient energy in the molecules of chlorine. The union with hydrogen takes place by reason of this stored up energy, and the rate of chemical change is *ceteris paribus*, proportional to the amount of efficient energy accumulated. An inhibitor is supposed to discharge the efficient energy of the chlorine, which loses thereby its power of uniting with hydrogen'.

With F. H. Gee the reaction between chlorine and carbon monoxide was studied, and two points were established; the photochemical reaction showed retardation effects similar to those observed for the hydrogen-chlorine system, caused by similar additions, and the thermal reaction at higher temperatures, shown to be substantially homogeneous in character, was unaffected in rate by these inhibitors. These facts probably played some part in explaining Chapman's reluctance to assume a photochemical dissociation of the chlorine as the first reaction step. Long after Nernst's chain mechanism

had been suggested even M. Bodenstein clung to an 'activated molecule' theory. In 1923 a statement was published which plagued the subject for several years. A. Coehn, G. Jung & H. Tramm examined the effect of drying on the reaction, using an impressive technique designed to eliminate the introduction of inhibitors. They alleged that although water-vapour in the ordinary way had no effect on the rate, after extreme drying the reaction was inhibited. It was impossible to reconcile this with the Nernst chain mechanism. A number of investigators had later to devote much careful work merely to prove that this inhibition effect was a myth due to some unknown cause. In the intervening years Chapman put forward an activated molecule theory which took into account the supposed effect of water-vapour and also gave a good representation of the experimental kinetic expression for the photochemical rate as obtained in his laboratory by Mrs Chapman (1923). This expression indicated, among other things, that the inhibitive effect of oxygen vanished at low hydrogen concentrations.

In 1924 Mrs Chapman carefully studied the effect of light intensity on the reaction rate, and her results showed that observed rates were slightly less than directly proportional to the intensity. Two years later A. Berthoud & H. Bellenot discovered that the photochemical iodine-oxalate reaction had a rate proportional to the square root of the light intensity. These investigators further showed that if the light was reduced in intensity by a rotating sector wheel the rate depended on the speed of rotation. Chapman immediately saw the importance of this work, and (with F. Briers & E. Walters 1926) re-examined the reaction with special attention to the latter observation. The complete mathematical theory of the 'sector effect' was worked out and used to evaluate the mean life of the 'catalyst or chain'. It is unfortunate that in so complete and authoritative a book as *The photochemistry of gases* by W. A. Noyes & P. A. Leighton (Reinhold 1941) no credit is given to Chapman for this achievement which has subsequently proved so useful in kinetic studies of polymerization. He soon afterwards applied the new method to the hydrogen-bromine reaction, which M. Bodenstein had recently shown also to follow a square-root light-intensity law. Under the experimental conditions the mean-life of the 'catalyst' was found to be 0.063 s, its destruction being chiefly bimolecular. Chapman still preferred the 'activated molecule' theory to that of atoms, probably for the reasons mentioned earlier. A communication (with F. B. Gibbs) in 1931 further advanced the subject by stating that it had been possible to demonstrate experimentally that rates for hydrogen-chlorine mixtures in large vessels changed over under conditions of high purity to the square-root intensity law, while rates for the hydrogen-bromine system changed to the direct proportionality relationship when a powerful inhibitor, nitric oxide, was present. With P. P. Grigg a determination was made of the mean life of the 'catalyst' by measuring the photochemical reaction of hydrogen and chlorine in capillary tubes narrow enough for removal to occur wholly by first-order wall-reaction. Chapman now appeared to have recognized the claims of the atomic chain theory—'It may be stated

immediately that the results obtained in this investigation do not, so far as we can see, give any indication of the nature of the catalyst, and it may therefore, for the present purpose, be looked upon as a chain of alternately formed chlorine and hydrogen atoms, as Nernst and Bodenstein assume, or as unstable nuclei of unknown structure.'

A final paper on the hydrogen-chlorine reaction (with J. S. Watkins 1933) described measurements on the relation between hydrogen chloride and photo-sensitized water formation in mixtures rich in oxygen. It was shown that the results could be interpreted by a modified Nernst chain mechanism, but that although some suggestions by other workers could be eliminated the intrinsic complexity of the reaction prevented the logical deduction of a single set of reaction steps or a unique reaction-kinetic expression—'we think that in the present state of our knowledge an exhaustive examination of all the possibilities would be tedious and unprofitable'.

In his later period of work Chapman took up the subject of catalysis. Earlier workers had found that silver and gold were more active in promoting the combination of hydrogen and oxygen after they had been heated in hydrogen. This he showed was due to the formation by oxygen of an inactive oxide film on the surface; it also appeared, however, that at low temperatures oxygen also formed an oxide film having catalytic active properties. These effects were clarified by further work with metallic platinum. Active surfaces became inactive when reduced in hydrogen and then treated with oxygen at 1000 °C, and this was traced to the presence of an impurity, almost certainly copper, which forms an inactive and immobile oxide with oxygen but which diffuses from the interior to the surface during high temperature treatment in hydrogen. Experiments with metallic palladium showed that adsorbed hydrogen was not catalytically active, and that until this had been removed by oxygen the reaction exhibited an induction period. These careful experiments formed models for later workers on the catalysis of gas reactions by metals.

The last paper published by Chapman described work carried out in collaboration with his wife on the effect of water vapour on the combination of NO and NO₂. Here he refuted the results of H. B. Baker and his wife which had appeared to show that 'Baker-dryness' inhibited the reaction.

In the early part of the Second World War, until his retirement in 1944, Chapman worked with a small research team in his laboratory on secret work on gaseous diffusion for the 'Tube Alloy' programme.

Chapman married in 1918 Muriel Catherine Canning Holmes, eldest daughter of Samuel Holmes, M.A., Rector of Braunston, North Hants, who assisted him in some of his researches. Their daughter, Ruth Holmes, is married to Anthony Tomkinson, Lecturer in History at Royal Holloway College; there are two grandchildren, a boy and a girl.

Though somewhat of a scientific recluse, difficult to dislodge from his laboratory, Chapman played a full part in University and College affairs. He took a leading part in the introduction of a fourth year of research

(Part II) into the Final Honour School of Chemistry at Oxford. He served his appointed year as Senior Proctor, and was a member for a period of the Hebdomadal Council of the University and of other boards and committees. When called upon to act as an administrator, as in his ten years as College Bursar, he showed practical ability and great common sense. He had acted as Vice-Principal of Jesus College for a number of years before his retirement. He was elected to the Fellowship of the Royal Society in 1913, and served on its Council in 1934-6.

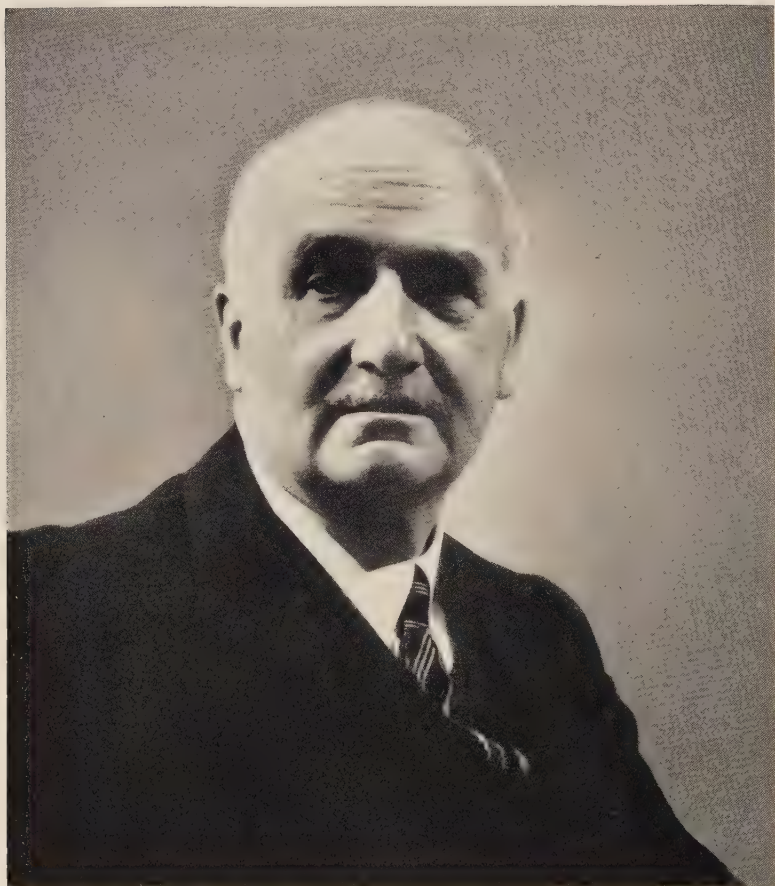
The writer is greatly indebted to Mrs M. C. C. Chapman for her assistance in preparing this memoir, and to other friends who have supplied information. The section describing the work on explosion rates was written by Dr J. W. Linnett, F.R.S.; this the writer acknowledges with gratitude.

E. J. BOWEN

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F. A. Lindemann.

Cherwell.

FREDERICK ALEXANDER LINDEMANN, VISCOUNT CHERWELL

1886-1957

FREDERICK ALEXANDER LINDEMANN, VISCOUNT CHERWELL, was born on 5 April 1886, at Baden-Baden, where his mother was taking the cure. His father A. F. Lindemann was a naturalized British subject of French-Alsatian origin and was an engineer who had played some part in the making and laying of one of the earliest transatlantic cables.

A. F. Lindemann was a keen scientist and had an observatory and well-equipped laboratory at his large and pleasant country house in Devonshire, where his family of two sons and two daughters were brought up by governesses and tutors. The mother was an American whose father was born British.

At the age of 11 F.A.L. was sent, with his elder brother,* to a Scottish school run by a friend of his father, which specialized in preparing army candidates. F.A.L. was an outstanding pupil with a remarkable memory, but took a pride in not working much. When at home he spent much of his time in the laboratory. In 1902 or 1903 his father was persuaded to send both the brothers to a school in Darmstadt where F.A.L. quickly acclimatized himself to German methods and passed into the 'Hochschule'. Here also he distinguished himself and was accepted by Nernst into his Physikalisch Chemisches Institut in Berlin, where he took his Ph.D. in 1910. His school and University days were by no means all work. He played a great deal of tennis at which he won many prizes, and later, as an Oxford professor, he competed at Wimbledon. Nernst is said to have complained that he got little work out of F.A.L. in the tennis season. He was in fact at a tennis tournament when World War I erupted, and only just managed to get out of Germany, leaving his first prize behind. Soon after reaching England he joined the team that Mervyn O'Gorman was collecting at the Royal Aircraft Factory (now 'Establishment') at Farnborough. About a year before he had spent some time at the University of Chicago and gave lectures there.

Lindemann's published work begins in 1910 when he was a student in Nernst's laboratory in the University of Berlin. The best of his scientific work was done between this date and about 1924. In 1910 Nernst's whole laboratory was working on what was then very low temperature physics, especially specific heats. I remember Lindemann expressing great admiration for the way Nernst swung the whole effort of his co-workers on to this when it began to show striking results, abandoning some quite promising experiments. The startling decrease in specific heat of diamond at the

* Now Brigadier C. A. Lindemann, also a physicist, to whom I am indebted for this information.

temperature of liquid hydrogen, so that the conception of temperature virtually has no meaning for diamond in this range, was a scientific sensation. Einstein's formula, explaining it in terms of the still not-generally-accepted quantum theory, was only partially successful (this is the formula which assumes a single characteristic frequency). Lindemann in his doctor's thesis on the law of Dulong and Petit showed that it fitted metals adequately but not some of the metalloids, e.g. sulphur. Nernst and he published a paper in which the formula was improved by allowing to an oscillator not merely the energy $h\nu$ but also $h\nu/2$. This gives a much better fit and also allows the specific heats of compounds such as KCl to be calculated from the frequencies of the 'reststrahlen' which Rubens had observed many years before. The tying together of heat and optics in this way was a striking success, and partly compensates for the arbitrary character of the assumption. Nor indeed was it quite so arbitrary as it now seems. The quantum theory was still fluid and it was not absurd to suggest, as they did, that $h\nu$ might apply to the kinetic energy of an oscillator and $h\nu/2$ to the potential energy. Later of course the Debye formula came along and the Nernst-Lindemann one was forgotten. Incidentally, Lindemann accepted the superiority of the former at once.

At the same time he was working on the connexion between the characteristic frequency and the melting point of a solid. He first showed, with Magnus, that there was an empirical relation between the two for a number of metals and metalloids, using for the frequencies those deduced from measurements of specific heat made by T. W. Richards. He then proceeded to rationalize this. He supposed that a solid melts when the amplitude of the atoms oscillating about their mean positions is enough to bring them into actual contact with one another. From this follows a relation between the frequency of the oscillation, its energy in terms of the temperature at melting point, and the gaps between the atoms. These last Lindemann tried to find from the dielectric constant using Mossotti's hypothesis. The actual idea of relating melting to the amplitude of oscillation of atoms was not new. It had been suggested by Sutherland* twenty years before, but in those days there was no independent knowledge of characteristic frequencies. It is doubtful if Lindemann knew of Sutherland's work. The new theory worked surprisingly well, both for compounds whose reststrahlen were known, and for elements whose characteristic frequencies could be found from their specific heats at low temperatures.

It was typical of Lindemann's mind to bring together ideas in this way from different branches of physics in an order-of-magnitude calculation. His mind was extraordinarily lively, and he also had an unusually wide knowledge of physics, including astronomy, and what is now called geophysics. He had a gift for picking out the essentials in a piece of physics, even if sometimes he went too far in ignoring the aspects of secondary importance. He was a most stimulating conversationalist on matters of physics, and one

* Sutherland. 1890. *Phil. Mag.* (5) 30, 318.

went away from a session with him feeling that he had rearranged all one's mental furniture and added one or two rather bizarre objects to the room. His versatility is shown by the extraordinary variety of the subjects with which he dealt. Thus while in Berlin he invented, with his brother, a glass transparent to X-rays made of lithium, beryllium and boron, which he patented for use as a window on X-ray tubes. He wrote on the selective photo-electric effect, and found a relation between the corresponding frequency and that of an electron moving in a Kepler orbit round one of the positive ions. He tried to improve the electronic theory of metallic conduction by considering the electrons as forming an elastic solid of high frequency. This gets over the difficulty of the specific heat, which was the great stumbling block of the old theory, it needs, however, some special assumptions to fit the facts. It contains the essential element that the electrons form a degenerate system, though of a somewhat different kind from that later considered by Sommerfeld. He also considered the theory of solids and the forces that must exist between the atoms. In doing so he called attention to the paradox that the breaking stress of all solids is much less than it should be, and considered that breakage must occur between crystals rather than across them. Before and during the First World War he published a few papers on astronomical problems such as the effect of radiation pressure on comets, the number of dark stars, daylight photography of stars, and the period of Cepheid variables. A more important paper, written with his father in 1919, was on the use of photo-electric cells in astronomical photometry. In addition to describing a method for making the cells, this paper contains the first account of the Lindemann fibre electrometer, his chief contribution to experimental technique, though in a form appreciably different from what it eventually took.

In 1923 he contributed two more papers to the *Monthly Notices of the R.A.S.* In one of these he corrects a very improbable conclusion of Eddington's on atomic physics, in the other puts forward the view, now of course quite untenable, that spiral nebulae are distant dust clouds.

During the First World War, while engaged at Farnborough on aeronautical work, which will be discussed later, he lived at an informal civilian mess kept in a house called Chudleigh. Here he met, among others, G. I. Taylor, W. S. Farren, B. M. Jones, F. W. Aston, E. D. Adrian, and the present author, all later Fellows of the Society. Aston was fresh from the Cavendish, where he had been working on positive rays as J. J. Thomson's personal assistant. They had just produced plates showing two or perhaps three parabolae for neon, and Aston was convinced that they were due to neon having isotopes, the first case of this phenomenon outside the radio-active elements for which Soddy had coined the name. Lindemann was mildly sceptical, and anyhow loved an argument, so there were frequent discussions. Lindemann always won on points, but Aston was quite unconvinced and came back smiling next evening. Probably Lindemann was more impressed by the arguments than he would admit, for he and Aston

collaborated in a paper on the possibility of separation of isotopes by varying methods. Lindemann was responsible for the theoretical part and Aston for the description of his experiments, unfinished because of the War, on attempts to separate neon by diffusion in pipe-clay. The paper showed that some degree of separation by differences of vapour pressure must be theoretically possible, though the effect might be very slight. Later, in a second paper, Lindemann considered chemical and vapour-pressure methods in more detail, and came to the conclusion that with zero-point energy the effect would be small. However, Keelson* and van Dijk in 1931 succeeded in obtaining the separation expected on Lindemann's theory by distillation of neon near the triple point, and in 1935 made quite considerable samples of gas separated to a substantial extent. This experiment was also of some importance in confirming the existence of zero-point energy. Another of Lindemann's ideas published during the war tried to account for radioactive decay, with its enormous range of lifetimes, as a probability phenomena depending on the approximate coincidence of a number of particles moving in orbits with a frequency determined by the energy of emission and $E = h\nu$. Combining this with the Geiger-Nuttall relation he found that about 80 particles were needed, a reasonable number.

His work on some geophysical problems was much more important. In a paper published at the end of 1919 he criticized some aspects of Chapman's theory of magnetic storms and in particular was able to show that a beam of electrified particles of one sign could not be the cause because such a beam would spread too much under the mutual repulsion of the particles, and charge up the earth too fast, there might, however, be a nearly neutral beam driven by the pressure of light. He then joined Dobson in a paper which marks the start of the modern theory of meteorites, in which by a consideration of the velocities of these objects and of the heights at which they started and ceased to be luminous, as recorded by various observers, they were able to deduce important consequences about the atmosphere; in particular that the temperature in the region of 100 to 150 kilometres altitude must be round about 300 °K and not 220 °K as had been supposed. Some of the features of this paper are no longer accepted; in particular, the view that a meteorite forms a cap of compressed air in front of it which considerably reduces the rate at which it can be heated is valid only at considerably greater densities than the authors supposed. The problem is still not entirely solved, even with the help of much improved experimental data and radar reflections from the trails, but the existence of a region somewhere in the region of 100 kilometres up, where the temperature is considerably greater than in the stratosphere, is now generally accepted.†

His electrometer was described in a paper written with Kelly in 1924. Though in essentials it is Kelvin's quadrant electrometer much modified, it is a break-away from the Dolezalek and Compton line of development to

* Keelson & Van Dijk. 1931. *Proc. K. Akad. Wet. Amst.* **34**, 42.

† c.f. Kaiser. 1953. *Phil. Mag. Sup.* 2, p. 495. Bates & Massey. 1940. *Proc. Roy. Soc. A*, **187**, 261.

very small size and great simplicity. It is still a standard instrument and has many important researches to its credit.

A piece of work that has had a considerable influence on later thought on the mechanism of chemical reactions is contained in a short paper in the *Phil. Mag.* of November 1920, and an equally short contribution to a *Discussion of The Faraday Society* in 1922 on the radiation theory of chemical action. An attractive theory had been put forward that the exponential variation with temperature of the rate of chemical reactions, especially monomolecular ones, was to be explained by the absorption of the internal temperature radiation of the substance, which was supposed to provide the activation energy. Lindemann refuted this view, at least in the case of the inversion of sucrose, by pointing out that the rate was not greatly altered by exposing the sucrose to bright sunshine. The wavelength effective could be found from the temperature coefficient of the reaction, assuming it caused in this way. It came out as 1.05μ , but the radiation of this wavelength is 5×10^{13} times stronger in sunlight than in the dark at laboratory temperatures. After disposing of some attempted explanations, he put forward his own view in the discussion. The difficulty in supposing that a molecule broke up merely when it received an extra amount of energy was that the chance of this happening would depend on the frequency of collisions and one would not get the variation with density characteristic of a monomolecular reaction. He got round this by supposing that the molecule needed to be in the excited stage, on the average, for a considerable time, large compared with that needed to set up the Maxwell-Boltzmann distribution. In such a case the actual rate of setting up the distribution, that is the rate of collision, would not matter within wide limits. Looked at in another way, if the density was large many active molecules would be produced per second, but many also would be destroyed, and the number of active molecules available for action would be a nearly constant fraction of the whole. Except that Lindemann envisaged the break-up as due to the centrifugal force of a rotating molecule, while modern theory regards the essential step as the acquisition of enough vibrational energy, this is essentially the view now held.

He had always been interested in some aspects of mathematics, and towards the end of his life Cherwell published two or three papers on the theory of numbers. In these, by very elementary methods, he reached results on the distribution of primes like those obtained by Hardy and Littlewood by highly sophisticated mathematics. The methods are not rigorous, though in the last paper written in conjunction with F. V. Atkinson they are made rather more formal. I believe that even when advanced methods are used rigor is not always achieved in this difficult field. The first of these papers is a note in *Nature* of October 1941 written when he must have been much concerned with other very different matters.

I am greatly indebted to Sir William Farren, F.R.S., formerly Director of the Royal Aircraft Establishment, for the following notes on F.A.L.'s work on aerodynamics.

LINDEMANN'S WORK ON AERODYNAMICS

BY SIR WILLIAM FARREN, F.R.S.

Lindemann was one, and in some ways the most remarkable, of the scientists who joined the Royal Aircraft Factory* about the beginning of the First World War. He contributed a summary of his 'Reminiscences of Farnborough' to the booklet produced in 1955 in celebration of the 50th Anniversary of the beginning of the Factory's history at Farnborough. He said he had 'no special knowledge of aircraft, and was posted to a Department to which all sorts of odd questions were referred which did not seem to fit into any special field'. His work was left very much to his own choice and his 'investigations ranged from detecting aircraft by sound, explaining why some cylinders with cast-iron fins cooled better than others, designing and making rate-of-climb meters, sun-proof doping for wings, compasses, automatic pilots, turn indicators, bomb-sights, accelerometers, range-finders for the Navy, a contraption to brush aside balloon cables, to occasional excursions into aerodynamics such as the cause of spinning'.

His work on the spinning of aircraft has attracted wide attention, though some obituary notices have not given an accurate account either of what he did, or of the reasons why it was so remarkable. The published records are incomplete, and unfortunately many of those who collaborated with him have died. However, with the aid of some of my own personal records, and with the help of one or two friends who were at Farnborough then and have survived, I have pieced together what I believe to be a true and fairly full account of his contribution.

This was of two kinds. First, by a simple argument from general principles, he deduced correctly the fundamental mechanics of the spinning aircraft.

In his own words, from the same source, 'anyone watching a spinning plane could see that the rate of turn did not increase on the way down'. The longitudinal axis of the aircraft was not far from vertical, and the rotation was round a vertical axis, so that the main relevant aerodynamic reactions, the normal force on each wing, must be in balance with each other. This force, which is not far different from the 'lift' of the wing, depends mainly on two things, the angle at which the wing meets the air towards which it is moving, and the square of the relative speed. From a simple consideration of the geometry, the effective *speeds* of the two wings were much the same, but the *angle* of 'incidence' of one wing, the 'outer' or leading wing, was obviously far less than that of the 'inner' or following wing. There seemed no way of reconciling this approximate analysis with observation, except by recognizing that the normal force on a wing, at a fixed speed relative to the air, does not increase continuously with increase of angle, but for small angles rises nearly linearly, reaches a maximum, and then falls rapidly. The angle at which the force is greatest marks the separation of the flow from the upper surface of the wing, the 'stall', which had already been recognized as the basic feature of

* Re-named the Royal Aircraft Establishment in April 1918.

the accidents which led to a spin. Lindemann inferred that the *mean* angle of incidence of the whole wing must be near the stall.

His second, and in some ways more remarkable, contribution was by his own experiments in flight, as a pilot, in many of which he was accompanied by an observer.* He explored the methods of entry into and recovery from a spin, which he had deduced as logically right. At the same time, his remarkable memory enabled him to record readings of instruments in the aircraft in spite of the disturbing effect of its rotation at a rate of about one turn in three or four seconds. From these readings, together with those obtained by the observer in the aircraft, or in the camera-obscura over which some of the later spins were done, his collaborators were able to calculate the essential elements of the manoeuvre, and to produce a theory which enabled its aerodynamics to be explained in terms of the general knowledge of the aerodynamics of those days.

This advance in our understanding of the kinematics and dynamics of a spin marked a change from the situation where it was regarded as something rather odd and generally dangerous to one in which it took its place as one of the possible manoeuvres of an aircraft (though not one which was of any great value). It was something to be avoided if possible, but one which a fully-trained pilot must be capable of dealing with if it arose as an emergency, such as, for example, after losing control in a cloud. But a full appreciation, both of the problem which it has always presented to those who fly, and above all of the situation which Lindemann faced when he undertook his experiments in flight, can be reached only if the extraordinary geometry of the flight path and attitude of the aircraft are recognized.

A typical spin on a B.E.2e aircraft, which was the type on which Lindemann began his work, had the following characteristics. The path of the centre of gravity was a helix with a vertical axis, with a pitch angle of about 60° to the vertical. The inclination of the body of the aircraft was roughly along a tangent to the helix. The impression produced on the pilot was that the aircraft was nose-down *with its axis almost vertical*. He had a good view of the ground below, which was rotating at a rate of one turn in 4 seconds. It was, however, very difficult to get an idea of where the axis of rotation was in relation to the aircraft. But the single observation that *the apparent direction of the resultant of gravity and the acceleration due to the rotation was almost in the plane normal to the wings* (i.e. in the same direction as it would be in normal horizontal flight) made it clear that *the axis must be somewhere above the pilot's head*, or rather would be intersected by the line of his backbone prolonged. This was not the view generally held.

The misunderstanding was mainly due to the fact that the picture of the ground rotating relative to the aircraft conveyed to the eyes an impression that the centre of rotation was somewhere on or beyond the 'inner'† wing,

* R. McKinnon Wood, now Chairman of the L.C.C., was certainly one, and according to my memory David H. Pinsent often flew with Lindemann, but no records remain to prove this.

† In a spin to the left, or anti-clockwise, as seen by the pilot, the 'inner' wing is the left, or following, wing.

which would imply that the wing itself was more or less along a radius of the helix. This, however, would have produced a large centrifugal force in that direction. The observation that the resultant acceleration was in the plane of symmetry almost at right-angles to the wing implied that the aircraft was 'banked'* at almost 90° to the plane containing the tangent to the helix and the radius. This was found to be true of virtually all the spins of which records were made at about this time, the angle of bank lying between about 70° and 90° .

The span of the B.E.2e aircraft was about 40 ft. (wing tip to wing tip). The radius of a typical spin was also 40 ft. The speed of the centre of gravity was about 110 ft./s along the helix, equivalent to a vertical rate of fall of about 95 ft./s, and circumferential speed of about 55 ft./s. The corresponding time for a turn was about $4\frac{1}{2}$ seconds, the normal acceleration along the radius of the helix about $2\cdot5 g$, and the rate of fall about 430 ft. per turn.

It is difficult to make a diagram which shows these quantities clearly. In demonstrations Lindemann used small models, one of which I now have. The axis of rotation was a square rod twisted into the appropriate helix, on which slid a block on which the model of the aircraft was mounted at the appropriate distance and in a typical attitude.

Some published accounts have placed emphasis on the priority of Lindemann's work on spinning; it is appropriate to record here the available evidence, together with an extract from Lindemann's own summary of his work from the source mentioned above. 'The only merit I can claim in carrying out these experiments is that (unlike the professional pilot, who had usually not got a very good head for figures) I was able to remember the readings of the air-speed indicator, the bubble, the angle of incidence on the two wings (measured by tapes on the struts), the height of the beginning and ending of the spin, the time taken and the number of turns and to write them down in my notebook when I had straightened out the plane again. I am glad of this opportunity to correct some of the absurdly dramatic stories which have appeared about this investigation.'

The most complete statement of the whole investigation into spinning carried out at the R.A.E. in those years is contained in an unpublished report, B.A. 317, by H. Glauert (later F.R.S.) dated May 1919. A shorter report was published in the Reports and Memoranda No 411 of the Advisory Committee on Aeronautics, dated March 1918, the authors being Lindemann, Glauert, and Harris. Glauert's later report is more detailed, and contains much more experimental information, but the conclusions are essentially the same. It includes a valuable summary of what was then recognized as the history of spinning and recovery from it.

Glauert states that 'it is believed that the method of getting out of a spin was evolved first by Mr H. G. Hawker on a Sopwith aeroplane, but the manœuvre was not generally known until Major E. W. Goodden carried out

* This is consistent with the conventional use of the term 'bank' in the simplest case—a circular turn in a horizontal plane.

some spins on an F.E.8. The report of these experiments (dated August 24th 1916) is contained in Appendix I below. About this period also the spin became a recognized manœuvre in air fighting, and had a considerable value as a method of losing height rapidly or shaking off an opponent who had gained an advantageous position.

'The first scientific experiments on the behaviour of an aeroplane in a spin were carried out in the following year by Dr F. A. Lindemann on a B.E.2c aeroplane.' Goodden was the Chief Test Pilot of the Royal Aircraft Factory.

In 1916 there was a strong and growing feeling at the Factory amongst the engineers and scientists that it would be a great advantage if they could fly themselves as pilots, partly in order to be able to experience at first hand what flying an aeroplane is like, and so to understand something about it, partly in order to be able to do specific experiments either on the aeroplane itself or on equipment, gadgets, etc. Largely as a result of Lindemann's pressure permission was given for five or six to be chosen to go to the Central Flying School at Netheravon and Upavon. The first three were Lindemann, Keith Lucas and myself. We were followed later by the author of this notice, G. P. Thomson. I know that Lindemann flew the same types of aircraft as I did, namely Farman Longhorns and Shorthorns, Avro 504s and B.E.2cs, because we used to compare notes on them. We returned to Farnborough on the same day. To our great sorrow, Lucas was killed in a collision in flight at Upavon.

Having had the pleasure of living in the same house as Lindemann at Farnborough for some eighteen months, I had come to know him well, but I think the experience of learning to fly in his company was something quite unique. In the first place, it showed me what a great man he was, not that I had any doubt about it. He was six years older than I was in years, but many more in experience, both of the world and of science. But I doubt whether anything about him impressed me quite so much as his complete indifference to the difficulties of arriving at an R.F.C. Station in a bowler hat and carrying an umbrella. Lucas and I were in khaki, and therefore relatively inconspicuous, for which we were thankful. Lindemann was unperturbed, and, to our surprise, so was the R.F.C. Their instructions were to teach us to fly, and presumably did not extend to what particular kind of clothes we wore. Nevertheless, we felt it rather unfair that, purely because of our uniforms, we had to attend parades, and drill like soldiers, whereas Lindemann was naturally excused.

One thing I shall never forget was the way in which Lindemann passed the crude 'medical' test, namely an eye test, which was all that they bothered about in those times. The sight of one of his eyes was so poor that it was hardly an exaggeration to say that from the point of view of a test it was blind. It certainly hindered his flying afterwards, particularly in landing, since his stereoscopic vision was seriously impaired. When he came out from the test, naturally triumphant, I asked him how he had done it. His wonderful

memory, mentioned above, was the clue, and he said 'I took a 50-50 chance', and when I asked what he meant, he explained. 'I went in, and he sat me in the chair, and whilst we were chatting I used my good eye to memorize the letters on the side of the card which I could see. When he started the test, I knew he would either leave it that way round, or turn it over. He turned it over, so I shut my bad eye and read the new side without any difficulty. He then turned it the other way round, and I looked at it with my bad eye and read out the letters from memory. Of course, he *might* have insisted on which eye I used first, but I had to risk that.'

Lindemann's experiments on spinning were made in June and July 1917 on B.E.2e and F.E.2b aircraft. It is impossible to say how much flying experience he had had by then, and I never asked him. But from my own log-book it appears that I had then done about 80 hours' flying on half a dozen different aircraft. I had greater opportunity to fly than Lindemann had, and more interest in flying for its own sake, so it is probable that he had not done more than about 50 hours. His courage in undertaking systematic spinning, involving continuous spins of anything up to a dozen turns, has been widely recognized, but to anyone who has flown it is, to say the least of it, astonishing that he should have undertaken such an arduous and difficult, not to say dangerous task, after so little experience as a pilot.

Lindemann's conclusion, mentioned earlier, that the mean angle of incidence of the wing must be near the stall, led him to an exciting development of his insight into the whole phenomenon, namely the proof that the spin was merely a special case of the normal 'spiral glide', then commonly practised as a means of losing height, whilst still remaining roughly in the same part of the sky. He concluded that he could start a spin by putting the aircraft into a steeply banked turn, closing the throttle until it was doing a spiral glide, and gradually steepening the flight path and increasing the rate of turn by pulling the control column in and changing the bank to suit. Not only did he do this successfully many times, but he succeeded also in reversing the manoeuvre, that is to say in passing from a spin back into a spiral glide. This required great skill and sensitiveness, together with real familiarity with the aircraft and its controls, and moreover demanded great care owing to the possibility of overstressing the structure of the aircraft.

I am reminded by one of our companions* at 'Chudleigh', where some nine of us lived together for several years at Farnborough, of the occasion when Lindemann was taken skating by F. W. Aston (later F.R.S.), who was one of our company throughout the war. Lindemann needed much persuasion, because he said that he never liked to do anything in public which he did not do well. We went to Fleet Pond, and, supported by Aston, he reached the middle of the pond with some difficulty, to everyone's amusement. Once there, however, he proceeded to show how figure-skating should really be done.

He had a similar diffidence about his other accomplishments. Three of us

* R. McKinnon Wood.

went golfing with him, but again he needed much persuasion, as he said he had not touched a club since he wore a sailor suit. He did the first few holes in bogey, and the rest in one over bogey, having acquired a blister on his hand.

[*End of Sir William Farren's contribution.*]

Cherwell's strength as a physicist lay in two things: in his power of reducing a problem to its simplest form and in his wide knowledge of physics. His knowledge of mathematics was limited, but what he had he could use. He excelled at the flash of intuition, he was less successful in turning it into a steady flame. His most persistent attempt at an exhaustive study was of the quantum theory in its later form. His 'Quantum theory' 1931, which was followed and preceded by a number of shorter and more popular expositions, was an attempt to expand the idea that one of two conjugate quantities, as position and momentum or time and energy, is meaningless without the other. He thought that the difficulty lay in the wrong things having been chosen as fundamental concepts, we ought to have used charge and action not position and time, but he was not very successful in working this out, and a good deal of the book is just an expansion of Heisenberg's uncertainty principle. Cherwell was a true physicist in the sense that he demanded an intuitive theory, preferably one that could be visualized. He preferred statistical mechanics to thermodynamics in spite of his training with Nernst. It was natural for him to resent the purely mathematical formalism of orthodox wave mechanics, perhaps even he was right to do so, but he had nothing very effective to put in its place. The fact that he never worked on any one line, except on this impossibly difficult one for which his relative weakness in mathematics disqualified him, long enough to become wholly master of it, has reduced his reputation as a physicist, I think unfairly, but it was the defect of a quality which made him peculiarly well fitted for the two great achievements of his life, the work which he did in the war and afterwards as Churchill's scientific adviser, and the building up from almost nothing of the Clarendon school of physics.

As adviser to Churchill he needed a very wide knowledge of physical science, without being too biased towards any particular branch. He needed to be able to express the hub of a problem succinctly, and was all the better for not being too closely conscious of a multitude of less important considerations. Luckily for him, and for the country, he was the exact reverse of the painstaking researcher who follows out the line of his Ph.D. thesis till it is time to retire forty years later. How far Churchill recognized his special qualifications as a physicist one can only guess; he certainly recognized his powers of expression and of reducing a complicated situation to a few key figures. If the following story is not true in fact, it is so in spirit. When Mr Churchill was forming his 1951 Cabinet these exchanges took place:

'And then, of course, I must have the Prof. as Paymaster-General.'

Blank looks.

'Yes, I must. He is the adding part of my brain.'

Pause.

'No, that is wrong. I can add. What I can't do is substract (*sic*).'

In 1919 Lindemann was elected Dr Lee's Professor of Experimental Philosophy at Oxford. In 1920 he was elected a Fellow of our Society. The professorship carried with it a fellowship and membership of the Governing Body at Wadham, where he lived for a time. Later (1921) Christ Church, who had originally founded the professorship and retained some contact with it, offered him a studentship and rooms, both of which he accepted, but University regulations prevented him from becoming a member of a second Governing Body. While living at Christ Church he retained his full status at Wadham and sometimes dined there.

The Clarendon Laboratory, to which Lindemann succeeded, was a magnificent mausoleum. One of the first physics laboratories ever built in this country it had a curiously ecclesiastical air. His predecessor had filled it with the best optical apparatus of his time. When a new piece arrived it was unpacked, tested by the professor, usually at night, and then put back in its packing case. There were some twenty sets of nicol prisms, but no electricity of any kind. This was the province of Townsend in the other laboratory! When Lindemann was appointed he was almost entirely ignorant not only of the peculiarities of Oxford, but also of the general plan of English universities. It says much for his perseverance, his skill in choosing men and his political ability, and perhaps not a little for the broadmindedness of Oxford, that long before he retired, the new Clarendon which he had persuaded the University to build was comparable in importance with any physics department in the country. He did not do this through the normal way of setting up a school based on his own particular line of work and achieving pre-eminence in it. He did indeed try a number of ideas of his own, usually with a co-worker. The most successful of these, the work on meteors, and the electrometer, have been described above. But the researches undertaken were a rather heterogeneous lot with a bias, though not a very strong one, towards the nucleus. There was also a good deal of work on photo-electric cells of high sensitivity, in the construction of which Lindemann enjoyed using his high personal skill as a glassblower. Fortunately for Oxford, fear of the Nazis drove a number of Jewish and other physicists out of Germany in 1933. Lindemann took a prominent part in receiving them and got posts in Oxford for several men of outstanding ability, among them Professor Frank Simon, later Sir Francis Simon, F.R.S. This led to the establishment of a school of low temperature physics in which Lindemann, with his early training under Nernst, felt a natural sympathy, and which soon became world famous in its field. It did not indeed occupy nearly the whole effort of the laboratory which continued to turn out valuable work of varied kinds,

but it provided a centre and set a standard of technical and theoretical excellence which was helpful to all.

A substantial part of Lindemann's time and energy, especially in the earlier part of his time in Oxford, went in attempts to strengthen the position of physics in Oxford financially and administratively. He had a keen, though mordant, wit, and sometimes his letters on University business needed to be toned down by senior members of his staff to whom he showed them, but he was prepared to take advice. He could be severe in the laboratory. He was extremely sensitive to noise, especially when lecturing. On one occasion when a senior research man started singing in a nearby room while he was lecturing, he pressed the bell and told the attendant when he appeared to ask 'the operatic gentleman to give his performance outside the laboratory'. The attendant gave the gentleman concerned a nasty shock by saying: 'The Professor says you must leave the laboratory, sir!' He was hard to persuade of the need of expenditure where laboratory funds were concerned, but very generous with his own money, often helping both research students and staff in cases of sickness or personal trouble. He also gave the laboratory a sum of money to be used as a loan fund to help people over a difficult period, e.g. when studentships are paid in arrears.

The Professor's lectures suffered from a rather poor delivery, which made them difficult to hear at the back of the room. For this reason his undergraduate classes were small, but the material was excellent. In rather the same way, I am told, his speeches in the House of Lords seldom sounded as well as they read. All the same, his influence in the Clarendon was by no means limited to the research students and staff. The entry for the final honours school increased in his time from half a dozen to about seventy.

In 1937 he stood as an unofficial Conservative for an Oxford University seat, but was not elected.

One of the most interesting developments of the present age is the way in which science is acquiring political importance, as instance the effect on the United States last year of the Russian satellites. This is partly because of the assumed military importance of some of these discoveries and achievements, but only partly so. Scientific advance has become a matter of prestige, as art and scholarship were for a time in the Renaissance. For reasons which were to a certain extent personal and even accidental, Cherwell made science have more influence during the war than it probably would have had without him, and than it had in any other country. In this way he contributed quite appreciably to its present position.

He had met Churchill at the end of World War I. In 1932 he accompanied him as one of a party which followed the line of Marlborough's famous march from the Netherlands to the Danube. Three years later he joined Churchill in a cruise along the east coast of Spain to Tangier.

By 1934 it was becoming obvious that the defence of Britain from bombing in the event of war was a major problem. Lindemann wrote a letter to *The Times* urging the importance of research into it. In 1935 an Air Ministry

Committee of scientists, the Committee for the Scientific Survey of Air Defence (C.S.S.A.D.) started to study the problem, but Churchill had no confidence in the Air Ministry's judgement in the matter and wished the Committee transferred to the Committee of Imperial Defence, where also he thought its recommendations would be more influential. He and Lindemann had an interview with Ramsay MacDonald, then Prime Minister, who was sympathetic. When Baldwin became Prime Minister the Committee of Imperial Defence on Air Defence Research was set up with Churchill as a member. He insisted that Lindemann be a member of C.S.S.A.D. which reported to the new committee as well as to the Air Ministry. This arrangement did not work well and in 1936 the quarrel came to a head, members of the sub-committee resenting Lindemann's action in getting Churchill to support his views on the main committee where they felt that Sir Henry Tizard alone should represent them, and Lindemann had to leave though Churchill stayed on. In 1938, however, at his insistence, Lindemann was given a place on the main committee. It was, of course, this sequence of committees which was responsible among other things for the introduction of radar, then called RDF, which had been suggested by Watson Watt in February 1935.

Just before the outbreak of war Churchill, with Lindemann's assistance, wrote to Kingsley Wood, then Secretary for Air, on the possibility of a uranium bomb which had already been discussed in the press. The letter pointed out correctly some of the difficulties of constructing such a bomb, but went too far in decrying its possibility, as it missed the possibility of a reaction with fast neutrons. In this it erred in company with physicists of great renown, some of whom continued in this error for another six years!

When war broke out and Mr Churchill became First Lord of the Admiralty he took Lindemann with him as Personal Assistant, a post which he continued to hold when Churchill became Prime Minister, until he became a formal member of the Government as Paymaster-General at the end of 1942, by which time he had been raised to the peerage with the title of Baron Cherwell of Oxford.

As Personal Assistant his primary duty was to brief Mr Churchill on scientific matters. In Churchill's phrasing 'Lindemann could decipher the signals from the experts on the far horizons and explain to me in lucid, homely terms what the issues were'. But this was only part of his duties. Almost from the beginning he was consulted on statistical matters, and when Mr Churchill became Prime Minister (May 1940) Lindemann became head of an organization known as the Prime Minister's Statistical Section. It was staffed mostly by young economists from the Universities. When Lindemann, by then Cherwell, became Paymaster-General, it had also to advise him as a member of the House of Lords, as a Minister, and an attender of meetings of the War Cabinet, and was sometimes known as the Office of the Paymaster-General, but it continued to be primarily the

Prime Minister's personal section acting through Lord Cherwell as his personal adviser.

Mr Churchill was not by training or taste an arithmetically minded man, and Cherwell's first job was to present the quantities to the Prime Minister in a way he could assimilate. He had to summarize the monstrous mass of statistics thrown up by the administration so that the P.M. could see what was happening and grasp the essence of the big quantitative issues that arose for decision.

This meant charts and lots of them, and an endless stream of little minutes summarizing and analyzing Cabinet papers of all and every kind. There were albums containing charts about the war effort, about bombing, about output of aircraft and munitions, about the size and employment of the shipping fleets, and of the daily gains and losses from them. It was a regular thing for them all to be taken off to Chequers over the week-end and looked at after dinner; they were kept thoroughly up to date. These charts were things of beauty and ingenuity and just the kind of presentation the P.M. could understand.

In much the same way the 'Prof's' briefs reduced large tracts of official prose to manageable dimensions and identified weak points in the argument. He had a genius for summarizing and a splendidly simple unadorned prose style. He took the greatest trouble with anything he wrote, drafting, correcting, re-drafting and correcting again. He could be guaranteed to halve the length of any draft offered—so one who had excellent opportunities to know, reports—ejecting superfluous words and finding with obvious enjoyment two syllables to do the work of four. This brevity at times meant some sacrifice of accuracy and perhaps undue emphasis on one side of the case. But to members of his staff who had delved deeply in a problem and whose inclination, both as academics and as temporary civil servants, was to tell the whole story, Cherwell's reply was always *'l'art d'être annuyeux c'est tout dire'*. Brevity was an essential quality of minutes addressed to so burdened a man as Churchill.

Cherwell sent the P.M. an average of about one minute a day. They ranged over the whole extent of the war effort. A great many of them were briefs for the Prime Minister on proposals coming before the Cabinet, but many threw a spotlight on some relatively minor aspect of the war effort. Few were free from figures. An argument was hardly considered respectable unless backed by statistics. An attempt to establish a *prima facie* case for action by the P.M. had at least to be illustrated by appropriate orders of magnitude. On a few occasions the Prime Minister followed up directly the minutes by asking Lord Cherwell to submit a draft directive to the Minister concerned. More often, the minutes were sent to the relevant Department with a request for further information or for the Departmental view. Most Departments tended to resent the implied criticism; in many cases they considered that the minute represented an over-simplified reference to a complicated situation and that it had betrayed ignorance of many leading facts. This

view was expressed in the verse which circulated in Whitehall in 1942—

‘My secretariat scrutinizes memoranda topical,
Elucidating fallacies in detail microscopical;
I plumb the depths of strategy, I analyze ballistics;
Reform the whole of industry, or fabricate statistics;
My acumen’s infallible, my logic irrefutable,
My slightest proposition axiomatic, indisputable;
And so in matters vegetable, animal and mineral,
I am the very model of a good Paymaster-General.’

Nevertheless, even in those cases where the Departmental case was impregnable, it did no harm to the Department to have to explain articulately its point of view. In other cases, undeniable weaknesses were brought to light.

Later on, in 1941, the Central Statistical Office was established, but unlike the Section which continued till the change of Government in 1945, it was not concerned with the conclusions to be drawn from its statistics, still less did it make recommendations on policy. The Section provided the only complete central survey of the war effort which was available. This was the more useful as some of the service and production Departments were reluctant to produce statistics on any subject—particularly at short notice—where they could not furnish a figure accurate to the nearest unit. Lord Cherwell’s insistence on figures being used, which, if necessary, were only correct to within an order of magnitude, often caused alarm and despondency among those responsible for the activities under survey. His slide-rule became famous, he always carried it, and frequently used it in discussion.

The Section often reached the conclusion that departments were exaggerating the demands likely to be placed upon them and underestimating what could be done with the resources at their disposal. The detection in this way of excessive ‘requirements’ can often be as important in avoiding waste and maldistribution of resources as the forecasting of a shortage that will cause a bottleneck. The natural desire of departments to be on the safe side provides scope for an outside body able to examine the calculations critically. Sometimes it was necessary to call the P.M.’s attention to impending shortages known as such but on which not enough was being done. At the same time the least harmful method of meeting the shortage was suggested. This was often not what would have resulted from normal interdepartmental negotiations which usually end in a compromise and sometimes in ineffective half measures. On some occasions at least the Prime Minister did make proposals or issue directives based on the more forthright recommendations of Cherwell and his staff. These were always based on quantitative considerations; some measures though perfectly valid may produce less effect by orders of magnitude than others and Cherwell was never tired of insisting that a small change in a big thing may often be more effective, and is usually more desirable, than a lot of big changes in a lot of small things.

In the early stages of the war Cherwell was in strong opposition to the view held by certain groups that the war effort should be developed slowly, but later on when the swing was quite the other way he opposed what he thought to be excessive demands of the armed forces and the kind of austerity known as 'the death by a thousand cuts'.

About one third of the minutes to the P.M. were on scientific matters, but of course it is impossible to draw a sharp line. Of the others about 30 per cent. were mainly concerned with the armed forces, 20 per cent. mainly with shipping, 15 per cent. with food, agriculture and raw materials, about 10 per cent. with post-war problems, and the remaining 25 per cent. with miscellaneous topics ranging widely from the building programme to the shortage of matches, from economic warfare to the supply of doctors, from Russia and India to export policy, rationing, inflation and austerity. These proportions varied of course from time to time. The emphasis on post-war problems naturally increased during the last few years, and the proportion of the minutes dealing with the armed forces tended to decline.

An example of the criticism of accepted views which Cherwell often put forward, sometimes successfully, is his analysis in the summer of 1940 of the size of the German bomber force lined up against us. The official view gave the Germans a vast front line and a still vaster accumulation of bombers in reserve. Consequently the forecasts of the damage we were to sustain were quite hideous. Some instinct or fifth sense assured Cherwell that this was all wrong, and he hammered away at the Air Ministry and the Ministry of Economic Warfare about it for months, using good, bad and indifferent arguments and generating tremendous heat. But he finally won, and the conclusive engagements in the battle came when he confronted the Air Ministry with the inconsistency between the stock of aircraft and the scale of production over the past five years that the Germans were supposed to have had with the corresponding stock and production which we had. He ferretted out that our figures of current stock, past output and recorded wastage showed a 'gap' of (perhaps) 5000 aircraft. There were all sorts of wastage which we did not measure properly (e.g. aircraft sent for repair and never repaired), so that the wastage rates the Intelligence authorities were applying to Germany were far too low. Much of the German reserve was mythical, and even the size of their front-line squadrons was not all it was said to be. It required intense self-confidence, great persistence, an extremely thick skin, the P.M.'s protection (and eventually the services of a judge) for an amateur to upset the experts on a subject of this kind in the middle of a war: but Cherwell had them all—and the conclusion was of high strategic importance (*Second World War*, 3, 35-36).

Another and perhaps better known example is his analysis of bombing. He was an advocate of bombs with a high charge weight ratio (producing big blast effects), as opposed to the lumps of steel, with no more explosive in them than was required to produce plenty of fragments, which the Air Force were using in the early days. He went to look at the flash photographs taken

when the first of the new 4000 lb. bombs were dropped to see what damage they had done. The handful of photographs available showed stretches of open country. Cherwell concluded instantly that bombing must be most inaccurate. He would not be consoled by the accepted explanation that as the flash never went off simultaneously with the bomb burst and the aircraft was after all moving at speed it was not in fact really likely that the photograph would show the site of the explosion. He insisted that the photographs taken as a whole were a significant indicator of accuracy, and got the Prime Minister to arrange for a member of his staff to go to Bomber Command and make a full analysis of all the night photographs taken in recent months. His emissary returned with a somewhat shattering report which demonstrated fairly conclusively that (in these early days) our bombing was only a third or a quarter as accurate as anybody had realized. The conclusion was highly relevant to the question of research priorities, and led to a quick concentration of effort on navigational aids (*Second World War*, 4, 250). Again, considerable courage was required to insist on the discovery of such an extremely unpleasant truth. After all the truth would have come out in time: a lesser man would have left it to emerge.

Another of Lord Cherwell's contributions to the bomber effort was his insistence on the importance of more efficient bombs. He was a strong critic of the continued use of the general-purpose bomb which was available when the war started, and thought it a false economy to use up the stocks rather than scrap them. As Sir Winston Churchill has recounted in his *Memoirs*, Lord Cherwell was responsible for the introduction of the aluminized explosives as a result of a critical examination of the damage ratios claimed by the Ministry of Home Security for German bombs. This highlighted the fact that even at a late stage in the war our own explosives, either in the air or in anti-submarine war, were not as efficient as those of the Germans.

Cherwell did not believe in methods of beam bombing which he thought too vulnerable to countermeasures. He preferred target marking by trained crews using special navigation methods. He became a strong advocate of the device known as H₂S which presented to the bomber's navigator a radar picture of the ground over which he was flying. Such a device required high resolution and given the limitation on the size of the aerial imposed by the size of a bomber is only possible with 'centimetre waves', wavelengths of first 10 and then 3 cm being used.

In quite another field of the war Cherwell noticed that in reports of submarine sinkings the submarine was never sighted in advance but many ships reported escapes from submarines they had seen. He concluded that sighting the attacker was a matter of great importance. The binoculars issued to merchant ships were of indifferent quality because of the shortage, but he discovered an Admiralty store of instruments of the best quality held to meet the requirements of ships not yet built, and was able to arrange for some to be diverted to the merchant service.

In addition to his statistical section Lindemann, as he then was, set up an

experimental department, M.D.1, under Major-General Jefferis, first at Portland Place and then, when the house was bombed, at Whitchurch, near Aylesbury. Here he came on Friday afternoons to discuss what was going on and watch practice shoots. These were sometimes quite exciting for the others concerned, as it was difficult to persuade him to take adequate care and he would carry on explaining what he had in mind in exact phraseology while they were distracted by the high-pitched whine of metallic fragments. A good deal of the work was concerned with hollow charge weapons, using the principle of the Monroe jet on which fundamental work was done. He favoured them not only for anti-tank purposes but also for the breach of concrete defence and even against submarines. He strongly supported the use of a 35 lb. anti-submarine hollow charge bomb which was to be released in clusters from aircraft; Lord Cherwell strongly believed that the chances of a hit were much greater than in a single depth charge attack. The statistical data, however, did not convince the Ministry concerned who gave up the weapon after one inconclusive operational use. However, the rather similar weapon known as the Hedgehog, also partly developed at M.D.1 but not Cherwell's idea, proved successful and received his full support. Much more widely known was the projector infantry anti-tank or Piat, which was distributed to the forward troops, and which achieved some successes.

The original anti-tank 'sticky bomb' was made an effective weapon, but since the invasion of England, for which it was intended as a desperate weapon of defence at close range, never occurred, it was not used for its original purpose, though it found a place in street fighting for breaking an entrance through brick walls.

Lord Cherwell had a series of struggles with the Ordnance Board and with the War Office to get certain M.D.1 weapons accepted for service use. He tended perhaps to over-rate the increased probability of inflicting greater damage by some new weapon which required special handling in the stress of battle and to under-rate the damage to morale which could be caused by an accident to our own troops. On the other hand, he encountered from time to time a resistance to new ideas which amply justified belief in the existence of 'Colonel Blimps'. In one instance—that of a small anti-personnel mine developed at the time when Rommel's first advance in Cyrenaica indicated the deplorable absence of a reliable small mine on the British side—opposition continued to prevent the use of this mine for nearly two years, until the field for its employment had disappeared. The last of the series of objections made related to the quality of the ink used in the marking on the exterior of the case.

Though right in his controversy over the size of the German bomber force, Cherwell was less successful in another point of scientific intelligence, the V weapons. As a result of an air photograph of Peenemunde the view was expressed that Germany was preparing an attack with long-range rockets carrying a bursting charge of the order of 10 tons. This was taken very seriously and plans even proposed for the evacuation of London. Cherwell

reacted strongly; he maintained that the rockets were merely a cover plan to distract us from pilotless aeroplanes, the V.1's or 'doodlebugs' as they were afterwards called. He asserted that a charge of 10 tons was quite impossible and that even with a charge of 1 ton the Peenemunde rocket could not go 100 miles. This last point was of practical importance at the time, since they were expected to be used from stations in Normandy about 100 miles from London. In fact, of course, the rockets, V.2's, existed all right with a charge of 1 ton and a range over 200 miles, as was discovered even before they were actually used against us, by a team under the late W. G. A. Perring of the R.A.E., Farnborough, who reconstructed their design from the debris of one which landed in Sweden. Cherwell was right in denying the large charge, but as sometimes happened with him he was carried away by his argument into an exaggerated and untrue position. Yet it should be mentioned that in the heat of the controversy he insisted that R. V. Jones of Scientific Intelligence be called, knowing quite well that Jones held the opposite view to his own.

Among the developments which Mr Churchill pressed at Cherwell's suggestion were massed 'U.P. rockets for use as A.A. (the rockets had been developed by Crow before the war), the so-called aerial mines designed to entangle night-fighters and swing bursting charges round to hit them, and proximity fuses. Lindemann, as he then was, was also keen on the fluvial mine of 1940 designed to be floated down the Rhine to destroy barges, but vetoed by the French.

During the early part of the war, Cherwell was not much concerned with the quest for an atomic bomb. The M.A.U.D. Committee, which was the responsible body, had been started by Tizard as an off-shoot of his 'Scientific Study of Air Warfare' Committee. However, when the M.A.U.D. Committee reported in August 1941 in favour of the possibility of the bomb, Cherwell supported it in a minute to the P.M. and evoked the typical comment, addressed to Lord Ismay: 'Though personally I am quite content with the existing explosives, I feel we must not stand in the path of improvement, and I therefore think that action should be taken in the sense proposed by Lord Cherwell.' This meant, among other things, communicating the report to the U.S.A., and starting design work at Metropolitan Vickers on a scheme for isotope separation due to Professor Simon. After receiving Churchill's minute the Chiefs of Staff recommended immediate action and the 'Directorate of Tube Alloys' was set up in D.S.I.R. under W. A. Akers of Imperial Chemicals.

In October 1941 Roosevelt suggested that the work of development be jointly conducted by the two countries. By the summer of 1942 the favourable forecast of the M.A.U.D. Report had been confirmed, and at the meeting of Churchill and Roosevelt at Hyde Park in June the decision was taken for the U.S.A. to go ahead. Churchill* says that this decision was due to the progress that had been made in Britain.

* *Second World War*. 4.

Cherwell was present at the Atlantic Conference in August 1941, and at Washington in 1942. He also took part in the Quebec Conference in 1943, which produced the famous Quebec agreement between Britain, U.S.A. and Canada on the development of atomic energy, and was later disappointed that not all the results expected from it were achieved. From a second Quebec Conference in 1944 he was sent on to Washington by the Prime Minister to negotiate on the renewal of lease-lend, an assignment which produced some comment at the time but in which he was apparently successful.

During the last year of the war, relatively little was being done to produce new weapons on the Allied side, and Cherwell's limited scientific work was devoted to the study of German V weapons and other new German developments such as the hydrogen peroxide schnorkel submarine.

Much the greater part of Cherwell's efforts at that time was spent in serving on the many Cabinet Committees concerned with the approaching post-war problems. He was very keenly aware of the poverty which was bound to descend on the country the moment the war ended and he was anxious that we should take all possible measures to strengthen our financial position and to avoid the unduly rapid accumulation of welfare liabilities, both in this country and abroad. Many grandiose schemes were being put forward at this time for the assumption by the Allies of responsibility of guaranteeing standards of life in many of the countries which had been affected by war. He pointed out that if we advanced our responsibilities beyond those involved in the 'prevention of disease and unrest' as it was called, we should weaken ourselves permanently without having the ability to give lasting strength to others.

After the general election in 1945 he returned to Oxford and resumed his work as professor. The conversion of the Wykeham Chair at Oxford to Theoretical Physics on Townsend's retirement in 1946 was to a considerable extent the result of his efforts.

During the post-war period work was started at the Clarendon aimed at producing controlled thermonuclear reactions, first by J. L. Tuck and somewhat later by P. C. Thonemann. Tuck later went to the U.S.A. and is now head of a group working on this problem at Los Alamos. Thonemann's work was transferred to Harwell in 1951 and was the seed from which 'Zeta' developed.

While the Conservatives were in opposition Cherwell acted regularly as Opposition spokesman on economic matters. He moved an annual motion calling attention to the economic situation. The role of critic suited him, and these were the best of his speeches, ornamented with a pungent wit.

In October 1951 Cherwell returned to office as Paymaster-General with a seat in the Cabinet, and resumed his war-time role of special scientific adviser to Mr Churchill, the Prime Minister's Statistical Branch was set up again under him with a particular responsibility for 'the analysis and interpretation for the benefit of the Prime Minister of statistical data'. As

Scientific Adviser he dealt largely with military matters and the work was especially important during the first few months when Mr Churchill was Minister of Defence.

While in opposition Cherwell had been a consultant to the Ministry of Supply on atomic energy and a member of the technical committee, and on returning to office was asked by the Prime Minister to assume a general oversight of work in this field. Cherwell indeed considered that the main purpose of his return to office, apart from assisting the Prime Minister generally, was to secure the setting up of some non-Departmental organization which would take over from the Ministry of Supply. Apart from a dislike on principle of a Government Department running an industry, he felt that the Ministry of Supply were slow in producing a bomb, and were not likely to develop nuclear power as fast as he thought desirable. Other reasons for wanting a change were to make it possible to pay higher salaries to the top men than was possible inside the Civil Service and to provide better security. There was a good deal of opposition, and the delay was one of the main reasons why his period of office, which was originally to have been one year only, was extended to two. He was even prepared to have sacrificed, if necessary, his professorship, which he would have had to give up if he were away more than two years, to bring this change about. However, in April 1953 the Prime Minister announced that a Committee under the late Lord Waverley had been set up to examine the problem of transferring responsibility for atomic energy to a non-Departmental organization and stated that the Government had decided in principle that this should be done. It was only when the Government had accepted the Waverley Committee report that Cherwell finally resigned. The Committee's White Paper was published on 10 November 1953, a few days after his return to Oxford. He retained his connexion with atomic energy developments for some months as unofficial scientific adviser to Sir Winston Churchill, and was a founder member of the Atomic Energy Authority from July 1954 till his death.

Cherwell had strong views on technological education. He favoured the establishment of a Technological University on the lines of M.I.T., but was not successful in convincing the Government that this was the best solution. In fact, however, the large expansion of the Imperial College now going on comes very near to it. Undoubtedly Cherwell's advocacy of the expansion of technological education was a major force in bringing about the big improvements which have now been agreed upon.

He paid a visit to Australia in 1953 on business concerned with the supply of uranium. In the last months of his life he was closely involved in the controversy over the continuation of tests of hydrogen bombs, and the effects of radioactive fall-out. He spoke in the House of Lords in May 1957, in favour of continuing them, urging the importance of having an effective deterrent.

Cherwell would have been an arresting figure in any society. Tall and

saturnine with a deliberately cynical manner, it seemed somewhat incongruous that he should have been a vegetarian with distinctly faddy views on food (he would only eat the whites of boiled or poached eggs, though scrambled eggs were acceptable), who never smoked and never drank anything stronger than coffee, except when Churchill insisted on his taking a measured c.c. of brandy. It was perhaps less surprising to find that he was a good games player, in spite of his bad eye. The first impression that he made was often unfavourable, and indeed he had enemies. This was partly the result of a caustic tongue and a too-keen understanding of the weaknesses of others. Yet he was a success with undergraduates, whose society he liked, and to whom he was charming. A cocktail party, for a man who doesn't drink, must be something of an ordeal yet, though I cannot recall ever having seen him at one, I am told that he quite often went and was a most successful guest from a host's point of view, adding much to the gaiety of the evening. Certainly he was a welcome member of the Christ Church Common Room, where he normally went after dining in his own rooms. In spite of, or perhaps because of, his continental education and semi-continental ancestry he was passionately British in sentiment, and resented the accident by which he had been born in Germany instead of in England. He was admittedly wealthy (he left over £200 000) and made no attempt to apologize for the fact. He had no belief in equality, and ages before he could have expected to join it, defended the peerage on the grounds of hereditary talent. He had a limited number of friends, but of these he was very fond. As a young man he attended an early Solvay Conference where he met Maurice Duc de Broglie and started a friendship lasting all his life with him and his family.* He much preferred French society to German. Partly this was due to his hatred of orthodoxy and of red tape. He must have given the few devoted Civil Servants, whose job it was to look after him in his anomalous position, a very difficult time, but they seem to have liked it.

He played the piano for his own pleasure—mostly Beethoven and Chopin—but was shy of doing so in public though he played well. In his later years he spent a considerable part of each morning reading the newspapers. Photography was a hobby he rode for many years, starting with the early Lumière colour plates which he developed himself. Then he did more in black and white, but returned to colour with modern films.

People were black or white to him, no greys were allowed. This is not uncommon with women of strong wills but is perhaps more usually a feminine trait, if so it was the only one about him, unless his food fads count as such. In a sense, and in spite of his friends, who were not limited to men, he was a lonely man, but there was no sign that this was at all distasteful to him, rather the reverse. One got the impression of a man detached, unemotional and restrained, enjoying the insight his excellent mind gave him into so many aspects of life. But he was not so unemotional as one might at first have supposed.

* Which has the distinction, unique at present, of providing the Society with two Foreign Members.

Towards his 'master', Sir Winston Churchill, Cherwell had a deep feeling of devotion which showed whenever he spoke of him. He had himself the power of inspiring something similar; those who worked under him, though they might on occasion be maddened by his faults, came to view him with admiration and affection, much stronger than the feeling of loyalty which relations in a government department or in a laboratory normally engender. This power of leadership is remarkable in itself but doubly so when held by one so strongly individual as Cherwell. In fact, in spite of his superficial cynicism he was genuinely fond of those who worked for him and really interested in helping them. They could always rely on his support in all circumstances. He was a most fascinating companion when the time came to relax, funny, understanding and kind.

Though most of Cherwell's working life was spent as a professor—indeed, was not 'Prof' his nickname—he will probably be judged mainly by his influence on affairs in the war and after. Some of his ideas on weapons failed in their purpose, the use of rockets for A.A., for example, but they were reasonable ideas and in consequence found application somewhere. Thus the rockets found their place as weapons for air to ground attack on fighter-bombers. It is perhaps fair to say that he was better as a critic than as a creator. His most useful work was in revealing hidden errors and mistaken beliefs, as in the discovery of the inaccuracy of the early bombing. When he got an idea or accepted belief he would sometimes hold it with unreasonable tenacity against the facts, but if he had not had this kind of tenacity he would have been unable to make an impression on the fierce wartime world in which he had to work. It cannot have been very easy, for example, to bring about a cut in the ships going to the Middle East and America by more than a factor of 2 shortly before Alamein and 'Torch', but if this had not been done there would have been a disastrous shortage in Britain.

In his second term of office Cherwell noticeably mellowed, and was perhaps more influential in consequence. In one economic crisis it is said he was able to get his way though Sir Winston Churchill was on the other side, and it seems now that Cherwell was in the right.

Cherwell's work in the war was severely attacked; no doubt he made mistakes, started or sponsored projects which turned out failures, and was sometimes tactless in his dealings with others as zealous for victory as he was himself. But when all proper weight is given to these considerations one is driven to conclude that on balance the good far outweighed the harm. At a time when Churchill carried the burden of the world, Cherwell was of great assistance to him. This assistance went much beyond help on purely scientific matters, it covered indeed most aspects of the war, as is clear from the examples just given. The work he was asked to do was work which he was peculiarly well fitted for, by doing it he served his master well, and through him, Britain.

Cherwell became a Privy Councillor in 1943, was made a Companion of Honour in 1953 and a Viscount in 1956. In 1956 at the age of 70 he resigned

his professorship though he held it under status which made this unnecessary. He continued to live in Christ Church and died there in his sleep on 3 July 1957.

Reference has already been made to the help most kindly given me by Brigadier-General Lindemann and by Sir William Farren, F.R.S. I am also deeply grateful to a number of persons who have given me information about particular periods or aspects of Lord Cherwell's life, or have answered special questions. I have used the information so received very freely, and without it would often have been unable to proceed. Among these I should specially like to thank:

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G. P. THOMSON

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Walter R. Kelly

WALTER ELLIOT

1888-1958

WALTER ELLIOT was a scion of the Scottish Border Elliot Clan, many of whom were prominent in the turbulent times of border warfare before the union of Scotland and England, and after the union many rose to high distinction in Imperial affairs as soldiers or diplomats. One Earl Minto was Viceroy of India and another Governor-General of Canada.

Walter inherited the characteristics of his clan being distinguished as a soldier and reaching the highest level in politics. He had a powerful physique, with great physical and mental energy. His physical energy he worked off by walking and on every opportunity by swimming; his mental energy by reading science, philosophy, history, politics and indeed all kinds of literature, and by discussions with his wide circle of political, scientific and literary friends. He was an omnivorous reader. When travelling his luggage was weighed down with books. His extraordinary range of knowledge, his vivid imagination, his wide and facile expression enabled him to take a lively and leading part in discussions on almost any subject except sport and athletic games, in which he had little interest. His contributions to discussions which he enjoyed had original ideas on which he sometimes gave his lively imagination full play.

One may recall an incident which illustrates the restless physical and mental energy of the man. One bleak November day when he and the writer had nearly finished a long walk along the banks of the river Clyde which was in flood, he stopped, stripped and dived into the raging torrent, was carried about 50 yards down stream, clambered up the opposite bank, ran about 100 yards up stream, dived in again coming out near where he had started, and while drying himself with his shirt, resumed the discussion on the need for research on the inevitable social changes due to the aeroplane, the radio and other developments of modern science.

He lived to the full an intellectual life which to the great grief of his many friends was suddenly cut off while he still enjoyed undiminished physical and mental vigour.

His father, William Elliot, was a man of substance. In addition to owning the Estate of Harwood in the Borders, he was Manager with a controlling interest in the well-known Cattle Mart of Lawrie & Symington in Lanark. He was an outstanding figure in Scottish agriculture, highly respected for his ability and integrity. Walter's mother died when he was an infant, and he and a brother, who was killed in the 1914-18 war, and a sister, who married

Colonel Craig, an Ayrshire landowner, were brought up by a rather austere grandmother who was keen on religious instruction, which might account for his fluency in quoting scripture.

He was educated in Glasgow, first at the Academy and then at the University which he attended from 1906 to 1913, graduating in both science and medicine. In his time student life had a brilliancy and gaiety which has never been surpassed. Elliot and his life-long friend Osborn Mavor (James Bridie the dramatist) who also graduated in medicine, were in the centre of it all. They collaborated in running the *G.U.M.*, the students' magazine, which each for a time edited. Elliot was prominent in the Union debates which are conducted on the model of the House of Commons. He was President of the Socialist Society while James Maxton, who later became the leader of the Clydeside 'Reds' and was imprisoned for sedition in the 1914-18 war, was President of the Conservative Society. In these debates both Elliot and Maxton served their apprenticeship for their subsequent political careers.

While taking a science course Elliot became interested in research in the Physiology Department under the late Professor Noel Paton, F.R.S., and Professor Cathcart, F.R.S., where investigations on the physiology of nutrition were carried out by about half a dozen post-graduate research workers, and a few of the younger surgeons and physicians fitting in some research with their hospital and teaching duties. After graduating in science and while still in his medical course, Elliot was a frequent visitor to the laboratories, sometimes offering to help in the experiments and often offering suggestions. One of the investigations was on the etiology of rickets. He suggested that the research limited to the experiments on dogs kept under artificial conditions might be extended to farm animals living in natural conditions. He thought that 'bent leg' in sheep which he had observed on his father's farm on animals grazing on certain pastures under natural conditions was a form of rickets. Later, at the Rowett Institute, he had a free hand in investigating rickets and other deficiency diseases in sheep and other farm animals.

When an Officers' Training Corps was established at Glasgow University in 1908, Elliot was one of the first recruits, and when the war broke out in 1914 he immediately left his post as House Surgeon in the Western Infirmary and joined the Royal Scots Greys, a cavalry regiment, as a medical officer. He served in France and was awarded the Military Cross and Bar for conspicuous gallantry in action. After the war he continued his connexion with the army in the Reserve of Officers and in the 1939-45 war was Director of Public Relations at the War Office.

In December 1918 he was elected Conservative M.P. for North Lanark. He lost his seat in the 1923 election but was elected in 1924 for the Kelvingrove Division of Glasgow which he continued to represent, except for a short period when he represented the Scottish Universities, until his death in 1958, a political life of 40 years.

As an ordinary back bencher politics was insufficient to allow his mental energies full play. He renewed his interest in research, and during the recesses of Parliament spent most of his time at the Rowett Institute in Aberdeen which in 1920 was just being established with a small staff of junior workers in a temporary building.

At that time research in nutrition had entered on a new phase. Before the war it had been confined almost exclusively to energy exchange and the metabolism of proteins, fats and carbohydrates, the 'proximate principles' of food. Food requirements and food values were determined by physical and chemical methods. It was assumed that if people had sufficient food to satisfy hunger, the requirements of the body would be met. When Sir Frederick Gowland Hopkins put this assumption to the biological test in which animals were fed on all the known purified constituents, he concluded that in their natural condition there were unknown food constituents which he called 'accessory factors' essential for health. The practical importance of this discovery began to be realized when the investigations of Eijkman, of Grijns who first used the term 'deficiency disease' and Funk who coined the term 'vitamin', showed that the etiology of beri-beri was to be found not in the presence of a toxic factor but in the deficiency of a constituent of food in its natural state.

When the younger research workers in the physiology of nutrition returned from the 1914-18 war, they found that research on energy exchange and metabolism of the energy yield constituent of food had been largely replaced by the exciting work on vitamins which was revealing the etiology of prevalent deficiency diseases.

The Director of the Rowett Institute which was established in 1919 for research on the nutrition of farm animals, thought that grazing animals, and even those in confinement which were fed on whole grain or offal rich in vitamins, might be more liable to suffer from deficiency of minerals than of vitamins. So while doing some work on vitamins to keep in touch with the most important developments in research, he directed the main effort on mineral requirements, a subject almost completely neglected in the enthusiasm on vitamins.

In 1921 Elliot and the writer who had both been at the war but had not seen each other since 1914, happened to meet in London and spent the evening discussing the recent advances in nutrition. Elliot became interested and was invited to come to the Institute and do some research work during the long Parliamentary recess. He found a small, young, inexperienced staff, uninhibited by orthodox ideas, working in an ill-equipped temporary laboratory. His first investigation was on the calcium requirement of the pig, an animal whose very rapid rate of growth makes it of special value in dietary deficiencies. As there was no technical assistance he had to feed and look after the pig himself, collecting and analyzing the excreta. He complained, humorously, that he never thought he would be reduced to being a valet to a pig. The result of that and other experiments he wrote up in a

brilliant thesis for a D.Sc. degree, accorded in 1923, which was remarkable more for the novel ideas for further research than for the data he had accumulated.

Elliot's mind, however, ranged wider than animals in metabolic cages. The Institute had begun to investigate the mineral content of pastures, by far the most important food of ruminants. This research had been further stimulated by the work of Sir Arnold Theiler and Dr Harry Green, who, by the way, was a fellow student with Elliot in the Physiology Department in Glasgow University. After a long, fruitless search for a toxic factor in lambsickie, a disease of grazing cattle in South Africa, they discovered it was due to deficiency of phosphorus in the pastures and could be cured by feeding phosphorus-rich substances or inorganic phosphates.

Elliot co-operated in pasture research with great enthusiasm. With the late T. B. Wood, F.R.S., Director of the Nutrition Research Institute at Cambridge, and the Director of the Rowett Institute, he drew up a programme of research to determine (*a*) the seasonal variation and (*b*) the effect of fertilizers on the chemical composition of pastures, the collection of samples of pasture in different areas sent to Cambridge or the Rowett for chemical analysis and the effect of the addition of mineral salts administered as salt licks on grazings or added to the rations of sheep on experiments in confinement.

After he was given office in 1924 he had little leisure to take an active part in research but he continued to take a lively interest in it. In 1926 a political event gave him the opportunity to exercise his flair for promoting investigations. Mr Stanley Baldwin, the then Prime Minister, had found it impossible to get imperial preference by tariffs on food imports, and as an alternative established the now defunct Empire Marketing Board with an income of £1 000 000 to promote trade in agricultural products within the Empire. £250 000 of the fund was set aside to promote agricultural research on an Empire basis.

Elliot was the leading figure in this research committee which with great gusto initiated highly speculative investigations. It was inevitable that pasture research would get a boost. The Civil Research Committee of the Cabinet was induced to set up a sub-committee of experts to consider and report on the relationship between the mineral content of pastures and their nutritive value. The sub-committee with Elliot as Chairman included Sir Walter Fletcher, F.R.S., Secretary of the Medical Research Council, the Directors of the Nutrition Research Institute at Cambridge and Aberdeen. The Committee made grants to Research Institutes and Department of Agriculture in different parts of the Empire and did a little towards co-ordinating the work by getting the senior workers to visit each other. The expansion of this investigation led to the elimination of several diseases due to minerals, diseases of economic importance in pastoral areas especially in Australia and New Zealand, where some of the most important work was done. This, one of the many investigations promoted by the E.M.B., is

specially referred to, because it was the one with which he was most directly concerned.

The E.M.B. with so much money to spend on speculative investigations was too good to last. It was replaced in 1933 by the Imperial Bureau of Agriculture, now the Commonwealth Bureau, financed by grants from all Governments in the Commonwealth. The most important work of this Bureau is the publication of *Agricultural Research Abstracting Journals* which are now the leading and in some cases the only international abstracting journals in their subject.

With the abolition of the E.M.B., Elliot's direct contact with agricultural research came to an end. In 1932 he had attained Cabinet rank as Minister for Agriculture and later as Minister for Health and Secretary of State for Scotland, and his official duties left no leisure for scientific pursuits.

As a Minister he was able to apply his knowledge of food and agriculture to practical ends. For example, an experiment financed by the E.M.B. in 1927, on 1500 school children in Scotland, on the nutritive value of milk showed a striking improvement in health and physique of those receiving milk, especially in the children of poor families, of whom there were many because of the massive unemployment. Elliot put a bill through Parliament enabling local authorities to provide cheap or free milk to schools, a measure which was later extended to England.

In the early 1930's when the price of agriculture products was so low that many farmers were going bankrupt he established the Agricultural Marketing Board for the better organization of marketing farm products and stabilizing prices. The Boards helped farmers to get better prices and were of great value to Lord Woolton in carrying through his wartime food policy.

In 1940 Elliot left the Ministry of Health for work at the War Office as Director of Public Relations. After the 1939-45 war he never held office. He became a highly respected and influential elder statesman taking part in debates on major issues. He was sent by the Government as leader of delegations to Commonwealth Parliaments and to advise on the setting up of universities in West Africa. He was Vice-President of the Parliamentary Association for World Government and as a friend of Dr Weismann was interested in the establishment and development of Israel. In 1956 and 1957 he was Her Majesty's Commissioner to the General Assembly of the Church of Scotland, an office which he carried out with great dignity and acceptance.

These public services left leisure for radio talks and political discussions on television, and for writing articles which showed marked literary ability. A number of his radio talks have been published under the title, *Long distance*. Apart from his essays which have not yet been collected and published in book form and his research publications, the only book he wrote was *Toryism in the twentieth century* (1927), in which he drew on his knowledge of biology in establishing the thesis that the development of conservative policy adapting it to social changes was a beneficent evolutionary process.

He received many honours. He was made a Fellow of the Royal Society in

1935, a Fellow of the Royal College of Physicians in 1940. He received the Freedom of the City of Edinburgh in 1938. He was given honorary degrees by each of the four Scottish Universities and the Universities of Leeds, Manchester and South Africa. He was made a Privy Councillor in 1932 and a Companion of Honour in 1952. He was elected Rector of Aberdeen University, 1933-36, and of Glasgow University, 1947-50.

He married in 1919, Helen, a daughter of Colonel Hamilton, R.A.M.C. She died in the same year as the result of an accident. In 1934 he married Katharine, a daughter of Sir Charles Tennant, a woman distinguished in her own right who was awarded a D.B.E. for social and political services. He had no children.

BOYD-ORR

NOTE ON ELLIOT'S E.M.B. WORK

By Sir Stephen Tallents, K.C.M.G.

In the early summer of 1926 Mr Amery, then Secretary of State for Dominion Affairs and for the Colonies, formed under his own chairmanship the Empire Marketing Board. This new body was to steer the administration of a grant of £1 000 000 a year, which the British Government—in compensation for a promise of preferences on canned salmon and a few other commodities, which they had been debarred by an election pledge from fulfilling—had agreed to devote to ‘furthering the marketing of Empire produce in this country’. I well remember the discouraging estimates of the virtues and prospects of this scheme which reached me at the time in Northern Ireland from high official quarters in London.

The Imperial Economic Committee, on whose recommendation the Board was created, had suggested that a substantial part of the grant should be devoted to scientific research. The E.M.B. pursued this suggestion by appointing a Research Grants Committee. Walter Elliot, as Parliamentary Under-Secretary of State for Scotland, was an original member of the E.M.B. He served during its first three years as chairman of that Committee and thereafter, upon a change of Government, as one of its leading members.

Under less active leadership, the E.M.B. might have become a machine placidly rubber-stamping ready-made projects for research. As it was, while always studious to seek the best scientific advice available, it retained the initiative for itself; and in that spirit it embarked on an active and widely-ranging programme for improving the quality and diminishing the preventable wastages of the Empire’s agricultural products at home and overseas. It thus gathered about it a strikingly varied assembly of workers and counsellors, whom its opportunities united in an enthusiasm rare among official organizations.

No one contributed more to the creation of that initiative and the generation of that enthusiasm than Walter Elliot. He brought to the research

activities of the E.M.B. not only a scientific training but ideas and zest and gaiety. His mind—especially when refreshed by one of his frequent visits to Scotland and communings with John Orr, then Director of the Rowett Research Institute—bubbled over with ideas. His colleagues could have had no better example of the truth of that pregnant saying of William Blake's: 'The cistern contains. The fountain overflows.'

This is no place to catalogue or to illustrate by more than a few random samples the hundreds of grants, large and small, made by the E.M.B. for the advancement of scientific agricultural research. At home they included massive support given at critical stages to Hardy's work at Cambridge on food preservation and low temperature problems; to the work on fruit of Hatton at East Malling and of Barker and Wallace at Long Ashton; to Stapledon's work among the grasses at Aberystwyth and to the study of Empire timbers at Princes Risborough. Overseas they fortified at agreed points the work of the Australian Commonwealth Council of Scientific and Industrial Research and the similar New Zealand Department; the veterinary research of Theiler and du Toit at Onderstepoort and the development of the Imperial College of Agriculture at Trinidad. In less orthodox fields they enabled the establishment of a station at Slough for the study of the infestation of stored products and of the 'Parasite Zoo' at Farnham Royal. Year after year they made possible many oversea journeys for scientists in pursuit of their specialized interests.

In an altogether different field also Walter Elliot made a notable contribution to the work of the E.M.B. As chairman of a Film Committee, which it appointed in 1928, he played, in the teeth of much official opposition and other discouragements, a cardinal part in the original work which, in John Grierson's 'Drifters', started the British documentary film movement and demonstrated—in films such as Elton's 'Shadow on the Mountain', based on Aberystwyth, and Wright's 'O'er Hill and Dale', shot at Walter Elliot's own Border sheep farm—how valuable the film could be for conveying the results of scientific research and good management to those able to profit from them.

I like to believe that Walter Elliot, enjoyed as much as any of his other versatile public activities the opportunity which the E.M.B. gave him for the release and realization of ideas. I know that no one made a more original or fertile contribution than he to a task which, started from the ground amid pessimistic forecasts in 1926, earned towards the close of its less than seven years of life the signal honour of being described by General Smuts in a manuscript letter as 'the *one* bright spot in recent Empire policy'.

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A. J. P. Winsor

ARTHUR JAMES EWINS

1882-1958

ARTHUR JAMES EWINS was born in Norwood in 1882, his father being a signalman on what was then the South Eastern Railway. The increasing value of the estate of the old Dulwich College ('Alleyn's College of God's Gift') had enabled its Governors to found, in addition, a modern secondary school, Alleyn's School, in that neighbourhood. Ewins obtained his first important educational opportunity by winning a scholarship giving him entry to this school, at a time when its Headmaster was Dr H. B. Baker, F.R.S., who later became Professor of Inorganic Chemistry in the University of Oxford. By the time Ewins left school, therefore, in 1899, he had had a better opportunity than most lads of 17 in those days, to obtain a good grounding in chemistry, making him ready for a career in that subject. He left school, in fact, to accept such an opportunity at The Wellcome Physiological Research Laboratories, which had then recently been established in that part of London, at Brockwell Hall—a former manor house of which most of the estate had already become the public Brockwell Park, leaving the house and a few acres of land available to the Wellcome Laboratories for the remaining years of the lease.

Dr Walter Dowson, who was at that time the Director of the Laboratories, was chiefly concerned with the production and testing of antitoxic sera and other immunological remedies, to meet the requirements in that direction of the pharmaceutical business of Burroughs Wellcome and Co. That had undoubtedly been one of the main, immediate objects, which Mr. Henry S. Wellcome (later Sir Henry Wellcome, F.R.S.) had in view when, having become the sole proprietor of that business, he founded The Physiological Research Laboratories bearing his name. As the title implied, however, he had certainly intended that these Laboratories should eventually provide opportunities for researches extending widely beyond the practical needs of immunological production, and into fields of physiology, pharmacology and biochemistry, having no immediate or near relation to the interests of his commercial enterprise.

Although, therefore, most of the rooms in the house had initially been allotted to work connected with the testing, and even the bottling, labelling and packing, of sera and bacterial vaccines, there were two which had been left for research activities in the other directions mentioned, and had been provided with general equipment for chemical and physiological work respectively. These two laboratories were not large enough, indeed, to provide for the accommodation of more than one qualified research worker in each

case, and on a rather restricted scale; but the appointments had other attractive features for men who might wish to try their hands at whole-time and independent research with only a generally therapeutic aim, at a period when a range of opportunities of that kind, like that which is now available, was not even in prospect. Dowson had, accordingly, found it possible to attract to these Wellcome research appointments, in the earlier years of their careers, several men who were eventually to achieve distinction in research and to occupy important academic chairs, such as the late John Mellanby and the late George Barger. He had, further, had the idea of attaching to the laboratories of these qualified research workers, and to his own service of practical bacteriology and immunology, as high-grade technical assistants, or research apprentices, boys with good secondary school records in science; and several of these had come from the neighbouring Alleyn's School on Dr Baker's recommendation.

When I myself joined the staff of the Wellcome Laboratories as research physiologist, in 1904, Ewins had already been holding one of these research apprenticeships there for five years, during which he had been receiving training and gaining experience with more than one of the qualified research workers who were there before me. He had spent part of that period as assistant to John Mellanby, who, in 1904, was relinquishing his appointment there; and, when I first met him, Ewins had already begun his longer term of association with George Barger, with whom he was to work on a footing of increasingly intimate collaboration, until Barger, in turn, accepted a professorial appointment elsewhere. Barger retained for a few years longer, however, a consultative connexion with the work at the Wellcome Laboratories, in which Ewins was, by then, co-operating more closely with my own pharmacological programme.

And so it came about that, as a glance at the Bibliography on page 90 will show, practically all the scientific papers upon which Ewins's name appeared as an author, up to the earlier part of the year 1910, were joint publications by Barger and Ewins. These dealt with a variety of subjects, and the range of these gives an indication of the kind of opportunity which Ewins enjoyed, during these formative years, for acquiring an invaluable working knowledge of both the technical methods and the theoretical conceptions of organic chemistry, at that stage in its development, through his regular and intimate association with a chemist of Barger's wide experience and intellectual distinction. And it should be added that, in addition to those studies in which his share was recognized by joint publications, Ewins, during that same period, was certainly taking some part, though without such public recognition, in other investigations by Barger, which were concerned with one of the main currents of biochemical and pharmacological interest in the laboratories at that time—work on the curious drug ergot, its specific alkaloids, and then on the active proteinogenous amines which different extracts from it were found to contain. It was only at a later date, however, after Barger had gone elsewhere, that Ewins's name began to

appear on chemical publications dealing with further developments of that theme, in which he was by then playing a more prominent part.

As soon as circumstances permitted, the accommodation for researches in these Wellcome Laboratories, which had by then come under my own direction, had been substantially increased by the removal of all the technical procedures to a new and specially designed building. It had thus become possible for me to invite P. P. Laidlaw (later Sir Patrick Laidlaw, F.R.S.) to join the staff, as my own more immediate colleague in physiological and pharmacological research. And then, when Barger left us, I had to consider a staff appointment in chemistry. By that time, two of the recruits who had entered the laboratories some ten years earlier, as research apprentices, Ewins and A. T. Glenny, had become graduates in science at the University of London; and, apart from this graduation, it seemed to me that they had both, by their own work at the Wellcome Laboratories, established good claims to recognition as independent research workers, by promotion to the qualified staff. And it may, perhaps, be permissible for me, in that connexion, to recall the special thrill of satisfaction which I experienced when, some 35 years later, the Council of the Royal Society, during my term as President, but without any prompting from me, endorsed my earlier judgement of their potentialities, by nominating Ewins and Glenny for election to the Fellowship, in the successive years, 1943 and 1944.

Promotion to the staff brought Ewins into more immediate collaboration in research with Laidlaw and myself. As the Bibliography again shows, he was promptly at work with Laidlaw on the isolation of a previously unknown member of the series of the proteinogenous amines— 3β -aminoethyl-indole, indole-ethylamine, or tryptamine, the amine formed by the decarboxylation of tryptophane; and Ewins proceeded to produce this base also by an artificial synthesis. I have sometimes thought, during the past few years, that Laidlaw's largely forgotten description, at that early date, of its physiological actions may, at some points, have anticipated the discovery of the actions of the nearly related 5-hydroxy-tryptamine, the natural occurrence and activities of which have recently excited so rapid a growth of attention.

The relatively simple, natural bases of this series as a whole had, even at the early stage, been attracting attention, on account of their natural occurrence, and the possibilities of a physiological significance for some, at least, of them. Another of them— β -aminoethylglyoxaline, β -imidazolylethylamine, or histamine—had been isolated by Barger shortly before he left the Wellcome team, when he had been endeavouring to identify the substance responsible for the very powerful and characteristic activity of a particular kind of watery ergot extract. We had recognized that the process used in making this extract was such as to facilitate a maximal putrefaction, and the chemical reactions of the base we had isolated had strongly suggested that it was the amine from histidine. The fact that Professor Ackermann of Würzburg had, at the same time, been preparing this amine by deliberate putrefaction of a histidine solution, had then enabled us quickly to confirm

the identification of the base from the ergot extract. The physiological activities of histamine, through studies which began with those which Laidlaw and I had the opportunity of making at the time of its discovery, have since acquired a wide and familiar interest, especially in connexion with allergic and other less specific reactions to injury. Ewins was among the first to take part in the quest for more convenient methods of producing histamine, than that of extracting it from the more complicated mixtures of the products of putrefaction. With the late F. C. Pyman, then working in another of the Wellcome research departments, he found, by 1911, a clean, chemical method for the decarboxylation of histidine. Later in the same year he published, with Laidlaw, a paper on the fate of tyramine in the organism, based on experiments in which a solution of the base was perfused through a surviving liver; and in 1913 the same collaborators were to deal by other methods with the metabolism of tryptamine.

It will be seen that in this same period, from 1910 to 1914, apart from such work on the amines, Ewins accomplished quite a substantial body of researches on the properties and structural chemistry of some of the less familiar vegetable alkaloids, narcissine, damascenine, cytisine—the alkaloid of laburnum seeds, which Laidlaw and I had found to have a physiological action similar in very close detail to that of nicotine—and aspidospermine, one of the alkaloids of Quebracho bark, with the chemistry of which he was still busily engaged, when the outbreak of the First World War took us all by surprise.

At some date in the autumn of 1913, while I was still holding my appointment at the Wellcome Laboratories, I had received, for testing, a particular extract of ergot which exhibited, quite anomalously and, so far as I know, uniquely, a potent activity of the muscarine type. Obviously such an extract could not be issued for medicinal use; and it was one of the advantages of work in such an industrial connexion, that it enabled me promptly to commandeer the whole batch of this extract for research, and to hand it to Ewins with a suggestion that, since ergot was also a fungus, it might be reasonable to look for something like muscarine itself in this peculiar extract. In a few weeks of chemical fractionation, easily controlled by my biological tests, Ewins was able to isolate the base responsible for this action, as a few milligrammes of its platinochloride. Physiological trials, made on very high dilutions of this base in Ringer's saline solution, soon made it clear that it could not be the very stable muscarine, or anything closely related to it; for, the weather being rather warm, it was obvious that even in the weakly alkaline medium, and at the room temperature, the high dilution of the base rather rapidly declined in activity as my test was in progress. The suggestion of an alkali-sensitive ester was not far to seek; but with only a few milligrams in hand, of a substance which might never be found again, the prospect of our being able to identify it seemed remote. During the night, however, I suddenly recalled a brief communication, made some eight years earlier by my friend Dr Reid Hunt, then of the U.S. Public

Health Service, to a British Medical Association Meeting in Canada, in which he had described the acetylation of the natural base choline as producing an enormous, many thousand-fold increase in its characteristic activity. I well remember the feeling of eager excitement with which I hurried to the Laboratories on the following morning, to urge Ewins to acetylate some choline without delay; with the result that not many hours elapsed before we knew, beyond doubt, that the base which we had isolated from the remarkable ergot extract was, indeed, acetyl-choline. This is not the place to deal with the physiological developments which were to follow from that. Perhaps, however, I may be allowed to interpose a passing acknowledgement of the fact, that the isolation and identification, as accidental constituents of peculiar ergot extracts, of the two substances histamine and acetyl-choline, by my two friends of those early days at the Wellcome Laboratories, George Barger and A. J. Ewins, provided me with two of the main strings of my own research instrument, for the rest of my active years. For Ewins, the identification of acetyl-choline was to lead more immediately to another discovery of some interest, which removed an old-standing anomaly from the pharmacological record. Schmiedeberg and Harnack, some 45 years earlier, had published a classical account of the action and properties of the natural muscarine, in which they gave evidence of its aldehyde character, and concluded that it was the aldehyde corresponding to the basic alcohol, choline; and they had sought to reinforce this conclusion by showing that what they believed to be muscarine was produced, when choline was submitted to what they had assumed to be the oxydizing action of fuming nitric acid. This interpretation of the reaction had appeared to be further substantiated by a faulty analysis for the nitrogen content of the product; and this so-called 'synthetic' or 'artificial' muscarine had long been commercially available for pharmacological uses. It had quite early become known, however, that, in addition to actions resembling those of the natural muscarine, the 'artificial' product had others, which seemed rather to resemble those of nicotine; and, when I began to re-examine the actions of acetyl-choline, it soon appeared that these also included 'nicotine-like' actions, in addition its more immediately obvious 'muscarine-like' actions. It became obvious that the status of Schmiedeberg's and Harnack's product was due for re-examination; and Ewins was soon able to demonstrate, by qualitative tests and unquestionable analytical data, that it was not an aldehyde, but a nitrous-acid ester of choline—nitroso-choline. The way was then open for him to prepare other physiologically active, artificial derivatives of choline, including the nitric-acid ester.

These last two papers bore the date 1914; which meant, of course, that they appeared in the months during which the unknowing world was approaching the outbreak of the First World War. For Ewins, as well as for me, there had been opening, during those same early months of 1914, the prospect of a new kind of appointment. I had been invited to take charge of a Department of Biochemistry and Pharmacology in the new National

Institute of Medical Research, which was being planned by the then recently created Medical Research Committee (now Council). The opportunity seemed to be one which I could not refuse, especially as I was authorized to invite Barger to rejoin me as my chief chemical colleague, and also to bring with me his former associate Ewins, to the staff of the new Department, if he should be willing to come. And, in due course, the three of us took up these new research appointments, under a Government Committee, on 1 July 1914, when the possibility of an outbreak of war, little more than a month later, still seemed remote. When the shock came, and our expected research accommodation was promptly commandeered for use as a military hospital, we found good temporary quarters in the Lister Institute, where I myself was to remain, indeed, until 1920.

The war's demands naturally caused a dislocation of our research programmes, but certainly not a greater one than we desired. Some of the peace-time problems, which we had thought that we were leaving, found unexpected uses in the new emergency. To mention one example, not without a tinge of humour, I have mentioned the fact that Ewins had then been working on the alkaloids of the Mexican quebracho bark; and he had, indeed, just discovered, rather surprisingly, that one of the principal ones, 'quebrachine', was in every respect identical with an alkaloid already well known, from another plant source, as 'yohimbine'. The war had not been long in progress when I received through a medical scientist friend, then holding a commission with the armies in Flanders, an urgent appeal for full information about the poisonous actions of the alkaloids of quebracho bark, and for suggestions of antidotes. A spy had, apparently, brought in information suggesting that the enemy was using this bark to poison the water-supply. I happened to know that, apart from the rather vague therapeutic reputation of the young quebracho bark and its alkaloids, the old bark, rich in tannins and devoid of all but traces of alkaloids, was imported in large quantities into Europe, for use in the tanning industry. It seemed clear that the Germans had wanted, for other uses, some barges which they had found loaded with this bark, and had tipped it into the water just to get rid of it. It was Ewin's chemical identification, however, which enabled me to return a suitable reassurance to the source of the enquiry, and to kill a promising 'canard' before it was fully hatched.

The more serious problems, which soon gave us enough to think about, were concerned with the sudden disappearance of essential remedies, for the source of which this country had till then been satisfied to remain dependent on German scientific industries; and the outbreak of the war had, of course, stopped the supply of these like the turning of a tap. A specially acute problem was created by the sudden disappearance of salvarsan, when great new armies were beginning to be enrolled; and its replacement, by the efforts of British firms to improvise a notoriously tricky synthesis, needed and deserved all the assistance that could be given. Even in Germany this product had been under strict supervision, each batch being required to

pass a control test in Ehrlich's own Institute, before it could be issued. Ewins and I had to do what we could to replace this official control in Britain, the new Department, in which we had just begun our service, being the only one in the country which could be charged with such a public responsibility. It may be noted, incidentally, that this emergency provided the germ of a system of official biological controls for all the remedies requiring them, which was to be established after the war under the Therapeutic Substances Act. As experience was gained, and as the claims on my own time increased in number and variety, I was glad to be able to leave the biological as well as the chemical work required by this salvarsan control, more and more in Ewins's hands. This brought him into more direct contacts with the manufacturers, and among these was Mr Robert Blenkinsop, of the firm of May and Baker, who were sharing their interests in the salvarsan-substitutes with the French Rhone-Poulenc Company.

It was very natural for Mr Blenkinsop to be favourably impressed by Ewins's already very efficient handling of the chemico-therapeutic problems, a working knowledge of which he had been so suddenly obliged to acquire. The result was an offer to Ewins of a position with May and Baker, which would give him the opportunity of building up for that firm an essentially new research organization. I could not properly even try to influence Ewins against accepting such an offer; for, although I knew that I should feel the loss of him acutely, and especially at that juncture, I felt bound also to recognize that the national interest, as well as his own prospect, would probably benefit from the change.

And so ended, after twelve years, a close association, which had been not only entirely pleasant, but scientifically profitable for me, and in which Ewins had, I think, had opportunities for some kinds of experience which would not otherwise have been likely to come his way. Thereafter, although our friendly relations were never clouded by any misunderstanding, they were not, in the nature of the case, on the former footing of collaborative intimacy, either with me, or with the research colleagues who joined my own Department of the National Institute after his departure. Before he left us he had published a general method for the estimation of arsenic in organic compounds, which was to prove of immediate value to his own work and that of his staff in his new appointment, and also, of course, to many research workers for other firms dealing with similar war-time problems. But thereafter, during the remainder of that war, Ewins appears only to have published, and jointly with Barger, papers dealing with work left over, as it were, from their earlier collaboration in the Wellcome Laboratories.

A further glance at the Bibliography, indeed, will make it clear that Ewins's personal record of research during his remaining active years with May and Baker, as shown by the publications bearing his own name, alone or with others, was by comparison remarkably small. According to that record, he had apparently taken part, during the 12 years till 1916, in researches leading to some 36 publications, whereas his work thereafter, in

the 36 years till 1952, is represented by only six. We who were still in friendly touch with him, however, were well aware that this apparently restricted output did not mean, on the one hand, that Ewins was no longer taking a full personal part in the researches of his new organization, or, on the other hand, that he was predominantly dealing with matters confidential to his firm. The impression which we had formed is, indeed, more than fully confirmed by the Obituary Memoir which one of his research colleagues in this later period (Dr H. J. Barker) contributed to *Chemistry and Industry* (issue of 22 February 1958), where it is explicitly recognized that 'in the publications from his laboratories the name of Ewins has rarely been given other than a modest place among the acknowledgements'. It is not to be doubted, however, that the true position became sufficiently obvious, not only to the members of my own Department, but to the general body of research chemists in this country, and beyond it. The high reputation, indeed, as a leader of research, which, though he would have been the last to seek it, Ewins was to acquire with this chemical community, with medical workers specially interested in the progress of scientific chemotherapy, and even with a wider body of public opinion, is to be attributed, in the main, to what he was known to have achieved in this later period, when he was director of research for Messrs May and Baker.

There can be no doubt, I think, that in this position Ewins had found the kind of opportunity which was specially suited to his ability and his character. He had not, I think, the academic type of mind, with its insatiable desire to penetrate to the theoretical roots of a problem, or the pioneering instinct for penetration into really new scientific territory. He certainly had, on the other hand, qualities which were likely to be of greater importance for success in the main part of his career, namely, an exceptional promptitude in recognizing possibilities of practical developments from new discoveries made elsewhere, and an instinctive readiness to give his colleagues a full share in the interest and initiative required for such developments, and rather more than a full share in the public presentation of the results. Two instances of this promptitude may be briefly cited, which happened to come within my own range of observation.

The late Professor Warrington Yorke, in checking the curious finding of Professor Jancso of Szeged, that the reputed antidiabetic agent 'Synthaline' (diguanidyl-decamethylene) would cause the rapid disappearance of trypanosomes from the circulating blood of infected mice, had discovered that this was not, as Jancso had assumed, a starvation affect, but was due to a direct and specific toxicity of synthaline for the trypanosomes, which was demonstrable *in vitro* and with very high dilutions of the drug. When Professor Yorke came to see me, to report and discuss this observation, the late Dr Harold King, who had then succeeded Ewins in my Department, at once suggested that analogous diamidines should be tested for this action; and King and Yorke published, in due course, results showing that the diamidines were, indeed, even more effective than the corresponding

diguanidines. The opportunity for further developments was open to the world. Ewins and his team appeared, however, to have covered the whole range of likely ones, before anyone else had started; and the useful remedies stilbamidine (diamidino-stilbene) and pentamidine (diamidinopentane) were thus soon made available.

Another example of this promptitude in initiative came in connexion with the sulfonamide series. The original discovery of an effective chemotherapeutic agent against certain streptococcal infections had been made by Domagk in the Bayer Company's Laboratories in Elberfeld, with the red 'Prontosil'; and Tréfouel, Bovet and Nitti, in the Pasteur Institute, then showed that the simple, colourless sulphanilamide moiety of this, which was likely to be released from it in the body by a reduction cleavage, was, indeed, the effective therapeutic agent. General interest was greatly strengthened by Colebrook and Kenny's carefully controlled demonstration of this chemotherapeutic action on a uniform series of puerperal septicaemias. So far, however, the effectiveness of this new chemotherapy with sulphanilamide had been restricted to a narrow range of streptococcal infections, and the medical world was waiting for a development which would break through this limitation. Again it was to come from Ewins and his team. He had been both fortunate and enterprising, in making an arrangement with the late Sir Lionel Whitby, then in charge of the bacteriological department at the Middlesex Hospital, for a co-operative search for an effective chemotherapy of pneumococcal infections. The result was the discovery of sulphapyridine (pyridyl-sulphanilamide), the first of the series which now includes many more readily tolerated and still more affective derivatives of sulphanilamide. Sulphapyridine itself has, in fact, now long been largely replaced in therapeutics, not only by these later analogues, but, from an entirely different direction, by the antibiotics. At the time of its discovery, however, its introduction, not long before the outbreak of war in 1939, marked a definite and very important step in the opening phase of what was to become so complete a revolution in the treatment of many of the bacterial infections.

Rumours of the successful application of sulphapyridine in cerebrospinal (meningococcal) fever, as well as in pneumonia, had begun early to extend beyond scientific and medical circles; and an article in an evening journal had helped to spread its fame as a 'wonder-drug', even when it was still known only by its laboratory note-book number, as 'M & B 693'; with the result of a popular habit, in this country, of referring to sulphanilamide derivatives in general as 'M & B'—a habit persisting long after sulphapyridine, which originally gave rise to it, had been effectively superseded. And the practice was not exactly discouraged by Mr Winston Churchill when, broadcasting during the war his acknowledgement of the treatment, presumably with a then more recent sulphanilamide derivative, which had enabled him to recover rapidly from a threatening pneumonia, he referred to the remedy as 'M & B', explaining that the initials were, of course, those of his physicians—Moran and Bedford.

Ewins, in any case, and by general consent, had made an invaluable contribution to the progress of medicinal chemistry in this country, and especially to the development here of research on the chemotherapy of infections. All of us who had the privilege of knowing him as a friend and a colleague, and all, indeed, who were able to make any kind of effective contact with him as a man and a chemist, were united in admiration of and confidence in his essential soundness. He was a man of retiring disposition, modest and simple in his tastes and habits, straightforward in all his dealings, and in his ways of thinking. He was warm in his friendship, steadfast in his loyalty, open and generous in all his relations with his colleagues.

Ewins married in 1905, Miss Ada Amelia Webb, who survives him with one son and one daughter of the marriage. For assistance in preparing this memoir I am indebted to the son, Mr Harold Ewins; and Dr R. Wien, of Messrs May & Baker's Laboratories kindly supplied material for the Bibliography.

H. H. DALE

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J. H. Grace

JOHN HILTON GRACE

1873-1958

(F.R.S. 1908-1922)

JOHN HILTON GRACE was born on 21 May 1873 at Halewood, a village in South Lancashire not far from Liverpool, the eldest of six children of a farmer. He received his early education at the village school, from which he went on to the Liverpool Institute. Here he came under the influence of an outstanding teacher of mathematics, J. A. Owen (who lived to the advanced age of 103), and in 1892 he entered Peterhouse, Cambridge, with an open Entrance Scholarship. His was a vintage year for Cambridge mathematics; among those who took the Mathematical Tripos in 1895 were six future Fellows of the Royal Society; besides Grace himself these were T. J. I'A. Bromwich, F. W. Carter, B. Hopkinson, E. T. Whittaker (later Sir Edmund Whittaker), and A. Young. Young was to become closely associated with Grace in his later mathematical work. There was intense rivalry, not only between the candidates but between their colleges, for the Senior Wranglership. Grace, Bromwich, and Whittaker were the most fancied candidates, and the element of doubt in Grace's case was not due to any misgivings regarding his mathematical ability, but because it was thought that his rather unstable way of life might result in his not doing himself justice. At any rate a wag pinned on the door of one of the favourites, who was known to be of a religious turn of mind, the verse

‘Work, as if on that alone
Hung the issue of the day,
Pray that Grace may be sent down,
Work and pray!’

In the event, Bromwich was Senior Wrangler, and Grace and Whittaker were bracketed second. Rumour had it that Grace was in the lead at the end of the penultimate paper, but celebrated a day too soon. Whether this is true or not it is in keeping with the fact that Grace was his own worst enemy, and throughout his life he missed opportunities of achieving the greatness of which he was capable through his intemperate habits.

Grace became a fellow of Peterhouse in 1897, and was later a college lecturer at Peterhouse and Pembroke, and held one of the two University lectureships in mathematics then existing. He served on the Council of the London Mathematical Society from 1903 to 1916, being Secretary from 1904 to 1915, and subsequently a Vice-President. He was elected a Fellow of the Royal Society in 1908. He spent the year 1916-1917 as Visiting

Professor at Lahore, and from 1917 to 1919 he deputized for Professor H. M. Macdonald at Aberdeen, in the latter's absence on war service. He then returned to Cambridge, but in 1922 a breakdown caused his permanent withdrawal from academic life. For some years he lived in Norfolk, but later returned to Cambridge, where he lived very quietly until 1956. His last months were spent in Huntingdon, where he died on 4 March 1958.

An examination of Grace's published work reveals some unusual features. In ten papers published between 1898 and 1904 he reveals the topics which were to remain a permanent interest in his life. Then, at a time when a mathematician might be expected to be most productive, there is a gap of a dozen years, followed in 1916-1918 by half a dozen papers. These years, significantly, are these of his absence from Cambridge. The conclusion is irresistible that his teaching activities during the years he was at Cambridge absorbed most of his energies. He threw himself heart and soul into his teaching, and several of his pupils of that time have paid tribute to its outstanding qualities; he demanded the best of his students, and those whose interests lay in fields remote from his own found him difficult to keep up with. But to those whose interests lay close to his own he was immensely stimulating and suggestive.

After 1918 there is another gap in the record of publications, clearly due to the impending breakdown in health. But, from his retirement, he produced a steady flow of papers from 1926 to 1930, and two later papers, the work of his old age.

Grace was fundamentally a geometer; though many of his papers deal with invariant theory his outlook on this subject was much more geometrical than that of most algebraists of his period. His most important work was his collaboration with Young in the book *Algebra of invariants*, published in 1903, which first presented the work of Clebsch and Gordan to British mathematicians; an outstanding exposition of the subject. Many of his papers deal with various aspects of this topic, and they are characterized by great ingenuity and elegance. A fine specimen of his early work is his proof (1902), in the paper 'The zeros of a polynomial', that if A and B are points in the Argand diagram, representing two roots of the equation $f(z) = 0$, where $f(z)$ is a polynomial of degree n , then the derived polynomial $f'(z)$ has at least one zero inside the circle whose centre is the mid-point of AB and whose radius is $\frac{1}{2}AB \cot \pi/n$. The key to this result is the following lemma, for which he gives a simple and entirely characteristic proof: if $f(z)$, considered as a homogeneous binary form, is apolar to $\phi(z)$, then it has a zero in any circle enclosing all the zeros of $\phi(z)$. The idea of apolarity seems to have intrigued Grace, and he handled it in several papers with great skill, for instance, in his determination of the cases in which binary and ternary forms can be constructed with prescribed polar systems (1928), or in his proof that a ternary n -ic can be expressed as a symmetric determinant whose elements are linear forms (1927), or, again, in his very elegant generalization of Stroh's lemma to an arbitrary number of variables (1928). There is an epi-

grammatical flavour about most of his papers: generally a simple but striking and unexpected idea is introduced, which leads directly to the desired result. His last paper (1947), occupying barely a page, disposes of the possibility of an extension of Aronhold's theorem (that a plane quartic curve is uniquely determined by a suitable set of seven bitangents) to the sextic curve of intersection of a quadric and a cubic surface, by a simple remark, of which the referee observed that the idea was worthy of its author. Of interest, also, is his proof (1930) that a basis for the ideal formed by the polynomials in the coefficients of a binary form, which vanish when the form has some assigned projective property, is the set of coefficients of a number of covariants of the form.

The other main group of Grace's papers is more frankly geometrical. In his first, and only long, paper (1898) he exploits systematically the representation of lines and linear complexes by points and hyperspheres in four dimensions, and obtains very neat proofs of a number of 'chain' theorems for circles and spheres. It is from one of these theorems, incidentally, that he deduces the curious theorem on the 'extension of the double six'. If five lines in space have a common transversal, then any four of the five have a second transversal, and the theorem of the double six states that the five lines so obtained are themselves met by another line. Grace shows that if six lines have a common transversal, then the same is true of the six lines derived, by the double six theorem, from the six sets of five lines which may be selected from the given set of six. In another paper (1929) he shows, by ingenious use of invariance arguments, that the problem of constructing a rational normal curve of order $2r+1$ with four assigned osculating r -spaces is poristic, while that of constructing a rational normal curve of order r^2+4r+1 with $r+4$ assigned osculating r -spaces admits in general a unique solution.

Three isolated papers deal with the theory of numbers, two of these (1918) concern problems on Diophantine approximation and the third (1927) contains a simple proof of Lagrange's theorem, that any positive integer can be expressed as a sum of four integral squares, by an ingenious use of lattices. Mention should also be made of an elementary textbook on analytic geometry (published jointly with F. Rosenberg) well in advance of its time in its use of general methods, and his articles on line geometry in the tenth and eleventh editions of the *Encyclopaedia Britannica*.

Many of Grace's papers seem, at a time when general theories are the current mathematical fashion, to deal with very special and particular problems. Grace was, in fact, an amateur in the true sense of the word; he tackled a problem simply because he found it interesting. None of his papers are trivial, even when they deal with elementary topics. Most of them have a deceptive appearance of simplicity, since they depend on the exploitation of a quite simple idea. But to get these ideas, more than once or twice in a mathematical lifetime, one must have at least something of Grace's remarkable perception, which seizes on the really significant points in a

problem, and his instinctive feeling for what is, and what is not, really relevant. His uncanny facility is something not easily imitated.

As a man he was sociable and friendly, with a whimsical attitude to life, and a vigorous humour. His range of interests was wide; as a young man he was a keen rock-climber, and had a great love and expert knowledge of the Turf. His premature retirement removed a colourful figure from Cambridge life, and left behind only the memory of a very gifted and stimulating man.

It is not possible to mention by name all the numerous friends and pupils of Grace who have very kindly given me their impressions of him in his prime. I am, however, particularly indebted, for family details, to his nephew, Dr Alan Robertson, and to Professor H. W. Turnbull, F.R.S., and Dr W. L. Edge for sending me letters written by Grace.

J. A. TODD

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H. L. Guy

HENRY LEWIS GUY

1887-1956

SIR HENRY LEWIS GUY was born at Penarth, near Cardiff, on 18 June 1887, and died on 20 July 1956, aged 69 years.

He began his career as a pupil of the late T. Hurry Riches, Chief Mechanical Engineer of the Taff Vale Railway Company, in 1903. Studying at the Technical School, he obtained a scholarship to the University of Wales and Monmouthshire and for three years he studied under Professor A. C. Elliott, who was the head of the Engineering Department.

Guy took a triple course of Mechanical, Civil and Electrical Engineering and he obtained the University Diploma in all three branches, which was a feat indicative of his diligence and capacity for study.

In his early days, he concentrated so much on technology that he did not matriculate and this prevented him from taking the University of Wales degree of engineering.

This was made good in later years when he was honoured with a Doctor of Science degree.

Guy was one of the outstanding engineering students at Cardiff. Each year he carried off many of the high prize awards.

He was a deep thinker and absorbed in technical progress and although attracted to locomotive engineering he realized, even as a student, that the steam locomotive and even reciprocating engines did not offer a great future for more than a limited number of mechanical engineers and even as a student he made up his mind that the prime mover of the future was the steam turbine.

He concentrated on the study of the turbine with enormous success and even at that early age realized that the development of the steam turbine called for metallurgical progress and for perfection of ancillary equipment.

One of his contemporaries at the University comments that when there 'His ready help to all newcomers was typical of his attitude to young engineers. In fact for a young man he possessed extraordinary vision which made his colleagues at College look up to him with something akin to awe'. He won the Bayliss Prize of the Institute of Civil Engineers, a Royal Research Scholarship, and a Whitworth Exhibition.

On leaving the University of Cardiff, after a short period as a lecturer at the Technical College at Crewe, in 1910 he joined the technical staff of the British Westinghouse Company at Manchester and in 1915 was appointed Centrifugal Pump and Turbo-compressor Engineer. From 1916 he served as the Company's Turbine Engineer. In 1918 he became Chief Engineer of the

Mechanical Department of its successor, the Metropolitan-Vickers Electrical Company, a post which he retained until his resignation in 1941.

During this time he was responsible for inventions and researches directed to the improvement of steam power plant.

He was elected a Fellow of the Royal Society in 1936 and in 1939 he received the honorary degree of Doctor of Science of the University of Wales and the Honorary Associateship of the Manchester College of Technology. In 1938-39 he served on the Council of the Royal Society. Guy was appointed Chairman of the Engineering Sciences Sectional Committee of the Royal Society in 1940, and in 1941 the Royal Society invited him to join the Executive Committee of the National Physical Laboratory. Later he succeeded Sir Nigel Gresley as Chairman of the Engineering Research Committee of the National Physical Laboratory. Sir Henry was Chairman of the Committee of the British Electrical and Allied Industries Research Association, which organized the research work on the properties of steam, first under Professor H. L. Callendar, F.R.S., and later under Professor Sir Alfred Egerton, F.R.S. He was also appointed Chairman of the Power Plant Section of the British Electrical and Allied Industries Research Association, and a member and chairman of several committees of the British Standards Institution. In 1927 he was awarded the Thomas Hawksley Gold Medal for the paper which he presented to the Institute of Mechanical Engineers on 'The economic value of increased steam pressure', and he presented to that Institute a number of papers in connexion with power-station work. In 1939 he was invited by the Royal Society and the Institute of Civil Engineers to give the Fourth Parsons Memorial Lecture and was awarded the Parsons Memorial Medal. He contributed a number of papers to other engineering societies at home and abroad.

Just before the war in 1939, Guy was appointed a member of the Advisory Council on Scientific Research and Development of the Ministry of Supply, and later became chairman of its Gun Design Committee and its Static Detonation Committee (Bombs), and was chairman of the Committee on Armament Development for the Ministry of Supply; and chairman of the committee to report on the work and staffing of the Royal Aircraft Establishment; and chairman of the committee to report on the organization of aircraft armament research and development by the Ministry of Aircraft Production. He was also chairman of the Committee on Technical Organization of the Army Council. He was an Associate Member of the Board of Chemical Warfare and an Associate Member of the Ordnance Board until 1948.

In 1945 he received a C.B.E.

He was created a Knight Bachelor by His Majesty The King for his contributions in the engineering and scientific world and the announcement appeared in the New Year's Honours List published on 1 January 1949.

I came in close contact with Guy during 1934 when he brought to my notice the work of the steam turbine locomotives in Sweden and as a result

of a visit to Sweden and from data supplied by Guy, the L.M.S. Railway Company decided to build a steam turbine locomotive for which the Metropolitan-Vickers Company were responsible for the steam turbines and the gear drive. I found him a charming man to work with and most meticulous in compiling the necessary data which enabled me to advise the Board of the L.M.S. Railway Company that it seemed a promising proposal. The subsequent design for the power unit was a very sound engineering project and the engine worked very successfully on the main line service for the L.M.S. Railway.

Henry Guy resigned from the Metropolitan-Vickers Company in 1941, and was appointed Secretary of the Institution of Mechanical Engineers in October 1941, which position he held until his retirement for health reasons in 1951.

He joined the Institution as a graduate in 1906, became an Associate Member in 1912 and Member in 1927.

He served on the Council—first as Chairman of the North-Western Branch then as an ordinary member of Council and finally as Vice-President.

After he retired in 1951, Guy's health did not permit him to engage in any active pursuits, but he took up water-colour painting and got great pleasure from the art. His wise counsel and considered judgement did much to raise the standard of mechanical engineering in this country and it was largely through his guidance that the status of the Institution of Mechanical Engineers became accepted by industry, the Government and national undertakings, as a standard of high qualifications of professional engineers.

In preparing this memoir, I have had considerable help from the Institution of Mechanical Engineers, from Dr J. H. Gough, and from two of his old contemporaries at the University of Wales, Capt. W. Gregson and Mr P. L. Jones, all three of whom are past Presidents of the Institute of Mechanical Engineers.

W. A. STANIER



J. R. Hartness

DOUGLAS RAYNER HARTREE

1897-1958

DOUGLAS RAYNER HARTREE was born in Cambridge on 27 March 1897, and he died also in Cambridge on 12 February 1958.

His genealogy furnishes an admirable illustration of the principle of heredity in ability, for he could claim to inherit many of his gifts and his scientific tastes from one or other of his parents.

His father, who was a grandson of the famous Samuel Smiles, was for many years on the teaching staff of the Engineering Laboratory at Cambridge. He had retired from these duties in 1913, but on the advent of war in 1914 he took up work in the team led by Professor A. V. Hill which was studying anti-aircraft gunnery and cognate matters. This brought his son Douglas into the same team, and started him on the lines of doing numerical computation. After the war William Hartree co-operated with A. V. Hill in physiological experiments for a time, and then joined his son in numerical work. This he continued almost till his death in 1943, but the relationship was an exceptional one, because the son was the leader and the father ranked as the junior in this work.

His mother, originally Eva Rayner, was daughter of a prominent physician in Stockport, and sister of E. H. Rayner who for many years was Superintendent of the Electricity Division of the National Physical Laboratory. She herself was active in public service and in various women's organizations. She was at one time Mayor of Cambridge and she also served for a time as President of the National Council of Women.

These remarkable parents had three sons, of whom Douglas was the eldest. The other two unhappily have not survived. He was educated, first in Cambridge and later at Bedales School, Petersfield, from 1910-15. He attributed to the excellent teaching of this school, in particular in mathematics, the actual trend of all his tastes in later life. He entered St John's College Cambridge, as a Major Scholar in 1915, but at the end of his first year his course was interrupted by the war and he went into the team of A. V. Hill which has been referred to above. He returned to Cambridge after the war and graduated in 1921, having obtained First Class Honours in Part I of the Mathematical Tripos, but, no doubt on account of the disturbance of his career by the war, only Second Class Honours in Part II of the Natural Sciences Tripos. The details of his professional career were as follows. He was awarded the Ph.D. degree in 1926. He was a Fellow of St John's College 1924-27, of Christ's College 1928-29. From 1929-37 he held the Chair of Applied Mathematics in the University of Manchester,

and in 1937 there was a substantive promotion there into the Chair of Theoretical Physics, though during the period of the war he was mainly on leave of absence while serving the Ministry of Supply. In 1946 he returned to Cambridge as Plummer Professor of Mathematical Physics in succession to R. H. Fowler, and he held this Chair till his untimely death in 1958. He was elected into the Royal Society in 1932.

In 1923 he married Elaine, the daughter of Eustace and Beatrice Charlton of Keswick, who had herself been a pupil at Bedales School. They were not there together but it was the school link that first brought them into contact. She survives him. There are three children of the marriage, a daughter and two sons. They are all now married and have children. All have to a considerable extent followed the tradition of the family in their scientific tastes and employments.

In his personal character apart from his high intelligence I think modesty and unselfishness would be recognized as Hartree's leading traits. In general company he would be apt to be silent, unless the subject of discussion should chance to be one in which he was an expert, and then he could speak up clearly and lucidly, but not in the least aggressively. He was an admirable lecturer, and when he wanted to explain a problem to someone who was to work on it, he had a quick perception of the degree to which the listener was able to follow his talk, so that he could always make the subject easy to understand.

In the work he undertook his unselfishness was outstanding in the sense that he was always ready to help people with their problems, even at the expense of his own work. Actually this was perhaps a good thing, because there were a number of other people attacking the same kinds of problem in atomic physics as he was, so that the work would have been done anyhow, whereas there was no one in the world with the same gift for putting into shape *any* problem that was susceptible to numerical analysis. The unselfishness sometimes went to extreme lengths. Thus during 1939-45, when he was working with the Ministry of Supply, he had frequent occasion to travel between Manchester and London, and he always took with him some piece of calculation he had promised to do for somebody so as to employ the time in the train. It is related that after one such occasion he apologized to the man he had been working for on account of the small amount he had managed to get done, explaining that the train had been so crowded that he could not get a seat, and that in the corridor his elbow had been jogged so often.

His main interest was certainly in his work, but he also had strong outside tastes. One was for music which was a bond between him and the whole of his family. He played the piano, but his main activity in music was in an orchestra where he played the drums, and sometimes acted as conductor. Another strong interest was in railways, and in particular in railway signalling, and he records the fact that at the age when other small boys wanted to be engine drivers his ambition was to be a signaller. This interest had its

reward in much later life when he was consulted by the railway traffic staff on the question of the optimum running times for trains between Rugby and Euston, and was able to use both his general experience and his computing methods and instruments to give the answer. Still later he served on a Committee of the British Transport Commission and showed them how to use the new digital high-speed computing machines to solve the very complicated problems of traffic control, which previously had required months of work by the specialists in the subject.

In the many fields in which he worked, much that he did was necessarily in collaboration with others. It would hardly be possible to refer to all these in the detail of what is written below, but among his collaborators a few may be cited. The list includes the late J. R. Womersley, B. Swirles (now Lady Jeffreys), A. Porter, H. G. Booker and the late P. J. Daniel. I cannot attempt to apportion credit among these workers, and indeed I think that in some cases Hartree would have given most of the credit to his collaborators.

It is undoubtedly for his quite outstanding ability in the use of numerical analysis that Hartree's name will be best remembered. This began with the work he did on the technically new science of anti-aircraft gunnery in 1916-18. The science of ballistics has, of course, a very old history, but high-angle fire presented many new problems. His main activity was in the integration of the differential equations for trajectories, and in this at the age of 20 he introduced a reform which may not now seem very revolutionary, but which must have been rather disturbing for the older practitioners of the science. By tradition the angle of elevation of the trajectory had always been used as the independent variable, and the co-ordinates and the time were expressed in terms of it. He showed that there would be great advantages in using the time itself as the independent variable, and expressing all the other quantities in terms of that. He introduced this practice and it is now generally accepted.

After the war he returned to Cambridge for two further years as an undergraduate, taking the Natural Science Tripos, and he then continued there doing research under the stimulating influence of R. H. Fowler. He considered later that the most important event in his scientific career was a visit to Cambridge paid by Niels Bohr in 1921, who gave a course of lectures on the quantum theory. This was of course the 'old quantum theory', and it is difficult for anyone who grew up later under the 'wave mechanics' to realize what a curious period that was. It was both exciting and thwarting, for one had to believe in two fields of knowledge, each of which surely must be right, and yet they seemed mutually contradictory. It was under this stimulus that one of Hartree's earliest papers was done, in which, with a view to getting closer to understanding the light-quantum, he studied the propagation of an electromagnetic wave that was not uniform over the wave front. There were many other attempts going on at that time on similar lines, which were aimed at reconciling quantum and classical theories, but, as might be expected from our present knowledge, none of them had any success.

However, Hartree soon turned to a more profitable part of this field. The formula for the reflexion of X-rays from crystals can be made to give information about the distribution of electric density in the atoms of the crystal by means of Fourier analysis, and this type of problem was exactly suited to his skill. He also did work on various details of the Bohr atom model in the course of which he estimated the corrections that might be expected for various atomic energy levels on account of the inner shells of electrons. All this work was the germ that later flowered into one of his major contributions to physics, that of the Self-consistent Field.

With the advent of the wave mechanics, which was started in 1925, it became possible to feel confidence in much of the detail of atomic structure, and whereas previously the work had been mainly guided by the principles of general dynamics, now it came into the wave-theory. Second order linear differential equations replaced the earlier non-linear first order equations. The new work exactly suited Hartree's gifts, for it was in the numerical integration of differential equations that he had served his apprenticeship. In the new field the only problem that could be accurately solved was that of the hydrogen atom, but of course many efforts were being made to go beyond that, at first to such problems as the helium atom and the hydrogen molecule, and then to more complicated structures. He soon contributed to this subject by the introduction of the self-consistent field.

To see the importance of this, reference may be made to one of his much later writings, a *Report* for the Physical Society of 1948, where the subject is described, together with various extensions of it. The actual wave equations for the electrons in one of the heavier atoms are certainly not expressible in terms of ordinary mathematical functions. Some form of approximation is essential, and this would mean the construction of tables. 'It has been said that the tabulation of a function of one variable requires a page, of two variables a volume, and of three variables a library; but the full specification of a single wave function of neutral Fe is a function of seventy-eight variables. It would be rather crude to restrict to ten the number of values of each variable at which to tabulate this function, but even so, full tabulation of it would require 10^{78} entries, and even if this number could be reduced somewhat from considerations of symmetry, there would still not be enough atoms in the whole solar system to provide the material for printing such a table.'

The principle of the self-consistent field evades these quite insuperable difficulties by imagining that each electron is moving in a sort of smudged field due to all the others, and furthermore it takes advantage of the fact that in a heavy atom the inner electrons have filled up many of the lower-level groups completely, and the influence of their field on the rest is therefore comparatively easy to estimate.

As a start these fields are guessed and it becomes possible by the numerical solution of ordinary differential equations to find the wave function belonging to each of the electrons in these fields, which is a very much simpler problem than that of considering the simultaneous equations which would be

required to represent the atom's behaviour accurately. The computed wave functions for the electrons then yield an electric density which is likely to be rather different from its first guessed estimate, and from this density new values of the field are calculated. The calculation is done over again with the new field, and it must be repeated until the two processes of finding the wave-functions, and of their electric fields are mutually consistent. It will readily be believed that there are many opportunities for ingenuity in simplifying this work, by recognizing how large it is admissible to take the steps of the integrations and in other ways, so as to make the series of operations converge most rapidly to the final self-consistent results. Hartree had such a deep understanding of the subject that he saw quicker than anyone else how to take advantage of any short cuts that might offer themselves.

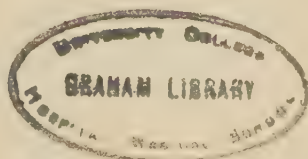
Working in this manner he attacked problems very much more advanced than others had attempted. For example, sometimes working in conjunction with his father, he solved the problem for such elements as copper and even mercury. All this work was being done with pencil and paper, but the situation was changed about this time, when Vannevar Bush developed his differential analyzer in America. This immediately excited Hartree's interest.

The differential analyzer is a machine which can do integrations by making a wheel roll on a rotating disk, while its distance from the centre of the disk is varying all the time. The principle of it was originally proposed by Kelvin, but in those days it could not work because the loads on the various shafts became so heavy that the wheels would slip. With modern electric techniques, however, a torque-amplifier can multiply up any mechanical force many times, and so it overcomes this difficulty, and the integrations can be done with considerable accuracy. The machine could obviously be adapted for the solution of differential equations, though in order to do this they have to be written in a form rather different from those usually given in the older text-books.

Hartree saw that this machine would much lighten the very heavy numerical work of his computations on self-consistent fields. He therefore made a journey to Boston to see the machine work, and on returning home he set up one for himself, the first model being made in 1934 of Meccano parts.* Starting from this, with the help of A. Porter, he obtained a number of results and later, again with his help and aided by a generous gift of £6000 from Sir Robert McDougall, in 1935 he had a much larger machine made. This was later copied in other places, but for long it was much the largest, best and certainly most used differential analyzer outside America.

It was a general purpose machine, and with his invariable generosity Hartree gave service with it to a very great many enquirers, and the service included giving them his intimate experience of the general way that numerical problems should be solved. Indeed, in some cases he could show them how with a little ingenuity it might be quite unnecessary to use the

* Meccano is a well-known toy with which school-boys can put together quite elaborate structures.



machine. One example of this arose out of the first German magnetic mine that was recovered. There were questions posed about its fusing and firing, and a team from the Admiralty went all the way to Manchester to ask Hartree for the help of his machine in solving them. After two hours' study he could give them the answers without using the machine at all!

His main purpose in making the machine had been to use it for the self-consistent field problem, but not long afterwards the problem itself was very considerably changed by Fock, who showed that what are known as the 'exchange forces' would play an important part in the matter. These forces arise because electrons cannot be treated as recognizable individuals, but are mutually interchangeable. They made very notable changes in the fields of the atom, and it turned out that they did not lend themselves so readily to solution by the differential analyzer. There was something of a reversion to pencil and paper, to which Hartree himself contributed again.

It thus came about that the most valuable work that Hartree did with the machine was the solution of a great number of general problems with a great variety of applications, rather than the purpose for which it was originally intended. I do not think there is any reason at all to regret this, because though Hartree made a contribution of first-rate importance to quantum theory by his work on the self-consistent field, he could only claim to be one among quite a number of workers who were making equally important contributions to the general subject, whereas in his particular gift for numerical calculation he was unique.

With the use of the differential analyzer, but of course very much more by his skill in seeing how it could best be used, he made important contributions during the next decade to a very great variety of subjects. Thus during the war he applied it to the study of anomalous radar pulses in the troposphere, to the behaviour of the magnetron, in particular to the effect of its 'straps', and to the steering of tanks. In the subject of anti-aircraft gunnery the matter of automatic following of the hostile aeroplane by radar devices gave difficult problems which were exactly suited to his gifts. One example that may be cited is a result on this subject which he obtained in collaboration with the late Professor P. J. Daniel of Sheffield, whom he had brought into the subject; it was forgotten when the war ended, and in fact was only rediscovered in America ten years later.

Apart from the solution of special military problems perhaps the most important contribution that he made in this field was to the subject which has now become dignified by the title of automation. This, of course, is not a new subject, for Watt had invented the centrifugal governor in the eighteenth century, and the theory of this device was worked out by Clerk Maxwell, but the new developments have been so great as to constitute a real revolution, and in this revolution Hartree played a conspicuous part. It began as early as the days of his original model differential analyzer. With the help of this he was the first to study fully the 'three-term controller', which has become the basic instrument for the control of chemical plants of

all kinds, so that now millions of pounds of manufacturing production depend on it.

The 'three-term controller' is essentially an instrument which controls the next stage in the production of something by measuring the past stages of its production; for example, it might be the control of the temperature of some chemical process. This might be determined either by the quantity that has just been produced, by its differential coefficient, or by some integral of its past production. For some manufactures one of these will be best, for others another, and often best of all will be a suitable combination of all three. In working out these things he made use of the invaluable Heaviside operational calculus, which was for long regarded with suspicion by many orthodox mathematicians, though more recently it has been recognized as identical with the method of the Laplace transform, so that it has become respectable. In particular it seems to have been he who, by the use of the operator e^{-pt} , first saw how to attack a practical difficulty that plays a great part in many manufactures, that of finite time-lag. This occurs when it is a considerably later stage in a long process which is to be used in giving the instructions that concern some much earlier stage. It would hardly be an exaggeration to say that very possibly, but for Hartree's work, a great deal of present-day manufacture would be different from what it in fact is.

The differential analyzer had scarcely become a well-established instrument when signs began to appear that it would be replaced by an even more powerful instrument, the electronic digital computer, and in the development of this Hartree again played an important part. The first such instrument, the ENIAC, was set up in America for the special purpose of calculating the trajectories of projectiles. Its potentialities had not been much developed by the end of the war, and one of the first acts of the designers was to send for Hartree so as to get him to advise them in its use.

I do not think it would be an exaggeration to say that it was he who taught them the way in which advantage could be taken of its extreme rapidity of action. The problem is formidable, as are all the problems which depend on two constants one at each end of a range of integrations. Here the two constants are respectively the initial elevation and the position of the target. With the older methods several trajectories had to be laboriously calculated from the start, some of which would go above the target and some below, and there then had to be a final interpolation to find the true trajectory. He showed them how to make ENIAC do one of these trajectories and at its end automatically make a correction for a new trial. It then repeated this process with new corrections again and again until it finally found the right path. All this was done without making any records, but as soon as the machine had the solution, it automatically received a new order, which was to go through the whole of this trajectory again, but this time printing out the answer. I believe the calculation of each trial trajectory took less than a second to do; the final printing was much the slowest part of the

proceedings, but even so the whole calculation was done in about half a minute.

The speed of working of electronic computers is very much greater than that of the differential analyzer and their accuracy is very much higher and the consequence has been that they have entirely superseded the differential analyzers in most of the work for which these had proved themselves so useful. The analyzers are still used for purposes of automation in manufacture, but even there they are being superseded in some cases, and in the research institutions most of the analyzers, laboriously installed a few years ago, are now lying idle because their work can be so much better done by the new instruments.

In this change Hartree played a leading part, largely by the advice he could give to those who were proposing to support the installation of the new machines. These included the EDSAC at Cambridge, which had been set up through the initiative of M. V. Wilkes, the Ferranti one at Manchester, and also many others including machines in America and Australia, both of which countries he visited as a consultant. Though he continued to the end to give advice in the use of these machines, and indeed often to help in the planning of the programmes for them in a great variety of subjects, he would not have claimed to do anything in the way of directing their use, and for this he would certainly have given all credit to their actual directors. When he returned to Cambridge in 1946 he was largely concerned with directing the researches of a number of junior men. These researches were in a great variety of fields, some of them concerned with atomic structures, and others with hydrodynamics, including questions both of diffusion and of the boundary layer; all these subjects demanded skill in the manipulations of arithmetic or in seeing how to adapt the machines to do this work for them.

In these later years I think it may be said that the central interest in Hartree's thinking was the adaptation of the electronic machines for dealing with partial differential equations. This subject had come up even in the earlier days of the differential analyzer, but there it had been a matter of deciding which of the two independent variables should have its differentials replaced by finite differences, while the other remained as a continuous variable. He had done some work for a steel-works on this matter in connexion with the cooling of steel ingots, and this work brought out the important differences according to whether it was time or position that was expressed in finite differences. Now with the electronic machine both had to be replaced by finite differences, and the centre of difficulty was the decision how big the steps might be taken for each of these variables. This is a technical matter to be studied for any problem before the machine can be used, and it is hardly possible to exaggerate its importance. Not long ago a case was cited of an equation which could be handled in two different ways according as to whether it was time or position that was considered first. Both methods looked equivalent at first sight, but it turned out that without any loss of accuracy the steps of the finite differences taken one way could be

no less than eight thousand times as great as if they had been taken the other way about. It will be readily appreciated from this example what an important part of the task is the arrangement of the lay-out of the work before it should be placed on the machine.

Partial differential equations constitute the central feature of many branches of mathematical physics, and the adaptation of such problems to the machines, much of it started by Hartree, holds the promise of leading to great advances in many subjects. In one respect fruit has already been borne, for in the old days the mathematician could in effect only solve linear equations and this meant a great limitation to the problems he could discuss, whereas non-linear equations continually present themselves, and the differential analyzer or the electronic machine can handle them just as well as the linear type. It may well prove that this freedom from the limitation to linear equations, a freedom which he was among the very first to gain, will grow into a transformation of the whole shape of the applied mathematics of the future. In his inaugural address as Plummer Professor in Cambridge in 1946 he said: 'It may well be that the high-speed digital computer will have as great an influence on civilization as the advent of nuclear power.'

One of the most important contributions made to mathematics by Hartree has been his books, and in particular the book entitled *Numerical analysis* which first came out in 1952; the second edition, amended in a few ways, only actually appeared after his death. Numerical analysis may be an austere subject, but he succeeded in producing a real work of art on it. This is partly because it calls not merely for deep understanding, but also for craftsmanship, and a craft is always interesting to contemplate for its own sake. Thus the reader, accustomed to mathematical works where the author is only concerned with the rigorous proofs of his theorems, and never thinks of looking beyond that, will find it most refreshing to read that everyone can always be sure of only one thing—that he will himself make mistakes sometimes. 'Anyone intending to undertake a serious piece of calculation should realize that adequate checking against *mistakes* is an essential part of any satisfactory numerical process. No one, and no machine, is infallible, and it may fairly be said that the ideal to aim at is not to avoid mistakes entirely, but to find all mistakes that *are* made, and so free the work from any *unidentified* mistakes.' All through the book attention is given to the best ways of detecting the mistakes that have been made. 'One kind of "check" is so inadequate as to be almost worthless, namely, repetition of a calculation by the same individual that did it originally. It is much too easy to make the same mistake twice . . .'

There are also a number of other examples where his practical experience contradicts the results given in most books. Thus he indicates that even the common formula for the solution of an ordinary quadratic equation with one unknown, as given in elementary books on algebra, is often nearly useless, in particular when the two roots are far apart. And again in dealing with the solution of linear simultaneous equations with many unknowns he says that

there are many different ways appropriate in various cases, but 'though there may be no one best way of evaluating the solution, it can be said with some certainty that the direct evaluation of the determinants and of the expression for the solution in terms of them is *never* the best way . . . ' although in fact this is given as the complete answer in many books on algebra.

An interesting general point in the book is that it almost unconsciously shows how much methods of computation have been changed by the use of machines, whether the desk-machine type or the newer digital machines. The older mathematicians who had to do numerical work would certainly have regarded logarithms as the key to such work, but logarithms have now gone so much out of use that, apart from occasional references to the slide-rule, they are never mentioned in the text, and in the index of this work there is not to be found any entry under the word 'logarithm'.

In writing an account of Hartree's life work it has not been possible to describe in detail a great many of the things to which he applied his gifts. They have been in so many different fields that to have explained them all would have taken any reader far beyond anything that would be likely to be his own special interests. The bibliography given below of Hartree's published works will show this and will enable anyone to discover anything in which he is specially concerned.

C. G. DARWIN

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F. Holm -

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FRANK HORTON

1878-1957

FRANK HORTON was born at Handsworth, Birmingham, on 20 August 1878. His parents were Albert Horton and Kate Louisa Horton (*née* Carley) and he was the eldest son and second child of a family of seven, five sons and two daughters; a most devoted family. Whether Frank's future career was directly influenced by the fact that his father was a schoolmaster and later an Inspector of Schools is not known for certain, but the future Professor and Vice-Chancellor may well have approached maturity with this background bias, which may have strongly turned his thoughts to administration in later life. As a child he got his early education at King Edward's School, Birmingham, where it was soon apparent that he showed real academic promise, so that he was encouraged to enter Mason College, Birmingham, where teaching and research prestige were already so high as to make the early attainment of University status a virtual certainty. His undergraduate career in Mason College, led in 1899 to a First Class External Honours degree in the University of London, in physics and in chemistry after which he chose to tread the path of physics research under the aegis of Professor J. H. Poynting. It was under Poynting's influence that the young Horton was first given the opportunity of showing, in the field of experimental research, those qualities of meticulous and painstaking care which, through all his life, were to characterize his work, whether in the laboratory, the lecture room, the administrative office or in committee.

When Horton became a research student at Mason College, Poynting's interest in the properties of matter had by no means been dulled by long years of study, indeed, the really beautiful experimental efforts to trap gravitation into yielding up her secrets, demanded, at the time, more and more sensitive apparatus which called for more and more knowledge of the elastic properties of wire and quartz suspensions. Such experimental investigations as this not very exciting field presented, were the first to be offered to Horton and a real testing time for his ingenuity and pertinacity they indeed provided. There is no record of any publication under his name during the two years he spent at Birmingham, though some of the work of that period must have been included in his first paper, published in 1904 under the title 'The effects of changes of temperature on the modulus of torsional rigidity of metal wires', a paper communicated by his new Professor, J. J. Thomson, to the Royal Society and accepted for the *Philosophical Transactions* on 1 March 1904. At this time he had been at Cambridge for several years, having gone up in 1901 as an 'advanced student' to St John's College with

an 1851 Research Studentship; what more natural than that Poynting should have sent so promising a young assistant to his collaborator Thomson (Poynting and Thomson's famous *Manuals on Physics* had begun to appear and were eagerly bought wherever physics was taught and learned) complete with a problem so obviously relevant to the gravitational investigations described in their joint and popular book *Properties of matter*. The problem was that of the properties of quartz fibres and some of the work then done at the Cavendish Laboratory will doubtless be remembered to this day by old friends. Like many techniques of the time, especially at the Cavendish, that famous home of improvisation, a certain simplicity, even homely ingenuity, was the rule and an example of this was Horton's method for measuring the diameter of extremely thin quartz fibres. He 'just' rested his fibre on a fixed horizontal glass rod, sandwiching it with a similar upper one, which could be moved slowly at right angles to the fibre length. As it moved, the fibre rotated and a marker on the fibre (actually it had been previously upturned at one end by softening in a flame) indicated the number of its revolutions; from the distance moved by the movable tube (usually about 5 mm.) the fibre diameter was easily calculable. Horton proved his capabilities as an experimental investigator in a long investigation into the torsional rigidity of quartz fibres, the results of which were published in the *Philosophical Transactions* of 1905.

These extremely accurate measurements on quartz fibres were carried out also on certain metallic wires, in particular reference to their changes in modulus of torsional rigidity with temperature. Horton realized that 'The effect of heat is to cause the crystalline grains of which a metal is composed to grow and this alteration of the structure would mean an alteration in the rigidity of the material. The rate of growth of the crystals at 100 °C would be very small and it has been observed only in the case of the softer metals but it is possible that the present method can detect changes which the microscope fails to show.' He then went on to discuss the effect of crystal size on the elasticity of a material like steel (rather in advance of the time) but primarily with the object of stressing the special value of quartz as a suspension material, in view of its freedom from such effects.

It is of interest to recall that this work was done in the Old Cavendish Laboratory, which was the original one made possible by the munificence of William Cavendish, seventh Duke of Devonshire, who was then, by great good fortune, Chancellor of the University. On the ground floor of this building there were four stone-flagged rooms, all of them well away from Free School Lane, and in the furthestmost, at that time, worked Professor J. J. Thomson and his assistant Everett: the honour of working in this same room had fallen to Horton, to whom the stone-topped tables, on masonry piers sunk well into the floor foundations, must have appealed, in view of the mechanical stability his work required. In the immediately adjoining rooms in this first decade of the present century, worked Norman Campbell, Butler Burke, T. H. Laby, E. M. Wellisch, H. A. Wilson, O. W. Richardson, J. A. Crowther, J. J. Satterly and many others. It is hardly surprising that

Horton, in such compact company, in spite of his natural somewhat shy and withdrawn young personality, should have made many friends. It was soon after his start at Cambridge as a research student, that he began to show that wider interest in people and things outside the laboratory which had tinged his earlier years and was destined to colour deeply his later ones. He had, as early as 1900, marked himself out as a student at Mason College by winning the Heslop Gold Medal for an English essay, and his facility for clear and easy writing made it natural, perhaps, for him to spare time from the calls a laboratory ever makes on the keen researcher, to editorial adventures. He was a pillar of strength to Sir J. J. Thomson as co-editor in the very considerable labour involved in the production of the *Monographs on physics* (known as the *Cambridge monographs*), some nine or ten of them having been published by Longmans Green & Co. by the time of the beginning of the first World War. Another service of this sort which he rendered as an editor was in the production, with Norman Campbell, of the well-known *History of the Cavendish Laboratory, 1871 to 1910*, published in 1910 in commemoration of J. J. Thomson's twenty-fifth year in the Cavendish Chair of Experimental Physics. Perhaps, however, his most colourful effort of this early period was a clever song in jingle verse set to the tune of *The tarpaulin jacket* and sung for years with appropriate gusto, together with other Cavendish songs, at the Annual Dinner. It may not be out of place to put it on record in this place. The version given, appeared in *The post prandial Proceedings of the Cavendish Society* as printed in its first edition of 1904.

'THE RADIUM ATOM'

Air: 'The tarpaulin jacket'

1 A radium atom was dying,
And just 'ere it burst up for aye,
Corpuscles which round it were flying,
These last dying words heard it say:

Chorus: Oh, I am a radium atom,
In pitchblende I first saw the day,
But soon I shall turn into helium;
My energy's wasting away.

2 About the laborat'ry latterly
I always have seemed in the way;
I've worried both Campbell and Satterly*
And many bad things made them say.

* N. R. Campbell and J. Satterly, On the use and abuse of radioactive needles for delicate electrometers. *Proc. Cav. Lab.* Oct. and Nov. 1904.

- 3 Electrometers get in a frenzy
When quietly by them I lay,
So they sent me downstairs with Mackenzie*
Who's wanting my last alpha-ray.
- 4 The theories about my interior
Proposed by the men of to-day,
Would make me appear quite inferior
—A crowd of corpuscles at play.
- 5 When wishing to be in the fashion
My gamma corpuscle they slay,
And state that according to Paschen†
'Tis merely a swift beta-ray.
- 6 To build up my weak constitution
I'm trusting myself to J.J.
He'll get the most truthful solution
Of all who have entered the fray.
- 7 My temperature's very unsteady,
I'm losing in weight every day;
But if the end's near I am ready,
At least, life's been lengthy and gay!

In passing might be mentioned his membership of a curious little club, confined as to membership to nine physicists and called the 'W.F.'s—the initials standing for 'we freaks'. The members met regularly in each other's rooms and held discussions on topics of interest, which were often the research interest of the host of the evening; in 1906 and 1907, for example, the records show that Horton talked about 'rigidity of wires' and 'Gravitation constant', immediate reflexions of his research interests at that time. But the Poynting influence was waning under the exciting new ideas springing from the workings of J.J.'s master-mind and Horton who, up to 1905, had been publishing work on gravitation and rigidity of wires, showed his new trend by publishing a paper in the *Philosophical Magazine* of 1906 entitled 'The electrical conductivity of metallic oxides'. He had served the older physics long enough and was now turning to the new. His interest in physics can in fact be related to the atmosphere in which he had his being; he served his apprenticeship, as it were, with the older classical physics under Poynting's influence, then for a few years he saw J. J. Thomson at work on his epoch-making positive ray analysis and the like, which started him on the second phase of his laboratory interest, namely the electrical conductivity of what

* A. S. Mackenzie, On the alpha rays from radium. *Phil. Mag.* 1905.

† Paschen, *Physikalische Zeitschrift*, 5, p. 563, 1904.

today would be called semi-conductors and the emission of positive ions and electrons from them, when heated. While at Cambridge he held the following scholarships and studentships: The Granville Scholarship (University of London); the Mackinnon Research Studentship, 1903 (Royal Society); the Allen Scholarship, 1903 (University of Cambridge); the Clerk Maxwell Studentship, 1906 (University of Cambridge). The work in this second phase lasted until about 1914 when he went to Royal Holloway College, Englefield Green, as a Professor of Physics in the University of London, where a new interest in physics gripped him, namely the fascinating topic of electron collisions with atoms and molecules, work in which he had great help from the students who were passing through his hands and which continued until his retirement from the Chair at Royal Holloway College. More than half of his forty-four published papers had to do with this last interest and were mainly in collaboration with students, in particular with Ann Catherine Davies, one of his earlier students, whom he married in 1939. The 18 papers published on thermionic emission and the like, between 1906 and 1914, and thus representing nearly a decade's work, are most difficult to summarize, since the various researches are more in the nature of interim signposts in fields afterwards cleared up with precision by successors who were fortunate enough to be in positions to command apparatus of greater efficiency; in particular the pumping equipment of the day which was so slow and imperfect in action that it is remarkable that results were obtainable at all! The work on electron emission from lime-coated surfaces, for example, came very close to the recognition of work-function, indeed in one paper, a table is given exhibiting the various amounts of energy required, under different conditions, to 'evaporate' a mass of electrons. There is another place where he recognized that conditions leading to the reduction of barium oxide to metallic barium led often to largely increased emission from the surface but he was not led to the recognition of metallic barium as a really responsible emitter. This step was taken by others many years later when, in fact, pump development had reached a much higher level and the resulting vacuum technique correspondingly improved.

In 1905 he had been elected a Fellow of his College, St Johns, and he took an active part in College affairs until he left Cambridge to take up his London University Professorship. During these years he frequently lectured for Sir J. J. Thomson at the Cavendish Laboratory where he also demonstrated in the Laboratory. He became Sc.D. of Cambridge in 1913, having taken the London D.Sc. some ten years earlier. During these earlier years in Cambridge, one of his closest friends was C. G. Barkla, a friendship renewed in later years when Barkla was External Examiner in Physics for London and Horton acted similarly at Edinburgh. Horton felt Barkla's death in 1944 very keenly.

A glance at the Bibliography will at once show the various topics Horton investigated while at Royal Holloway College: critical potentials for ionization, for radiation, for luminosity in gases and interesting results were also

obtained on secondary emission from solid surfaces. For several years he invaded the field of soft X-ray excitation, only to return in 1946 to publish a last paper on the ionization of helium by neutral helium atoms. It was in the earlier part of this period that he was elected into the Fellowship of the Royal Society in 1923.

One of the earlier papers from Royal Holloway College, with Miss Davies, was on a topic which may well have lain dormant in his mind from his Cavendish days and which was never really solved there by the many research students who worked fruitlessly at it under J. J. Thomson's inspiration. Published in 1920 and entitled 'An investigation of the effects of electron collisions with platinum and with hydrogen to ascertain whether the production of ionization from platinum is due to occluded hydrogen' (*Proc. Roy. Soc. A*, **97**, 1920). In pre-1914 days much work at Cambridge, including that of Thomson himself, had to do with discharge tubes, so that, naturally enough, the cathode, that source of the 'cathode rays' and the anode, the source of the 'positive rays', were of particular interest. The emission of the positive particles from the anode was regarded as due to electron bombardment but whether from the anode material itself, or from a surface film of gas, remained an open question which has been attacked from varying angles by different experimental methods. He may have been reminded of the importance of such an investigation through having experienced a complicating side issue which he and Miss Davies had run into, during research into the ionization potential of helium by electron impact. In this work, using the Franck and Hertz method of accelerating electrons through metal gauzes into a chamber containing the helium, he noticed a considerable positive emission from the gauze. His next paper expanded this observation into a most careful study of the source of the effect. He concluded to his own satisfaction at the time, that by bombarding a platinum surface with electrons, the ensuing emission, whether of radiation or positive ions, could not possibly be due to action on hydrogen attached to the metal surface. These conclusions were based on observations of the critical electron potentials involved and a comparison of them with the accepted values deduced from Bohr's theory.

A number of papers around this time in the early 1920's were concerned with radiation and ionization potentials in the rare gases and the importance of high degrees of purity of the gases under study; much of this work was of very real value at the time and contributed to the body of knowledge being built up in America and elsewhere on the same and related subjects. The remaining five papers on X-ray excitation are again typical of Horton's invariably careful and painstaking experimental skill and while they contributed considerably to contemporary progress and knowledge, the results have, as it were, been absorbed into the infinite. He, at the time, was keen on extending the experimental techniques developed at the Royal Holloway College laboratories during his studies of critical potentials to the field of X-rays. Such methods, he thought, quite rightly, would be useful in examining

X-ray excitations in the 'soft' region, a region which had already, to some extent, been successfully explored by others, using other methods. It was for this reason, perhaps, that this batch of papers may not have received the attention they deserved. Large numbers of most carefully reduced values for critical excitation potentials in this X-ray region were given for the elements iron, copper, cobalt and nickel. An earlier and even more detailed paper in 1926, had given similar experimental results on 'The soft X-ray absorption limits of certain elements' which had included not only the elements just mentioned but also chromium, manganese and zinc.

During this long period at Royal Holloway College the Physics School had been getting stronger and larger, while the facilities of his laboratories had been steadily improved: nevertheless he felt the lure of the wider administrative influences which could be wielded within the University of London itself, as distinct from those smaller spheres made up by the individual colleges. He accordingly allowed himself to be elected to the Senate of the University in 1925, which soon led to his membership and chairmanship of numerous committees—since his special aptitude for the efficient conduct of business soon became known and appreciated. It is certain that his influence in the expanding University of London was considerable and that he smoothed the way during the crucial and difficult times of decision which preceded the Act of 1926, under which the University was, in some measure, re-constituted. He became Dean of the Faculty of Science in 1930 and was elected to the Court in 1932, where he had a hand in guiding the finances of the University under its new constitution. He had served a term of four years as Chairman of the Academic Council till 1939, in which year he was elected Vice-Chancellor; an unhappy year in all ways and ushering in a period fraught with trouble, upheaval and misgiving for the University and its Vice-Chancellor. He assumed his new office within a day or so of the outbreak of war and the University was faced at once with all those difficult problems associated with dispersal and scattering to the various centres far away from the hazards of the bombing to come. At a time when all concerned felt sick at heart at the prospect of losing so much that had been built up over the years, Horton showed most admirable and unselfish devotion by keeping in the closest possible touch with the numerous evacuation centres, a task which involved him in a great deal of uncomfortable wartime travel. This work was enormously appreciated, so much so, that he was re-elected time and time again and ultimately held the high office of Vice-Chancellor throughout the war for six successive years; a proud record which is unlikely ever to be repeated. The Principal of the University of London in a report soon after Horton's retirement from his chair in 1946 said:

'No period of its history was more fraught with danger for the University than these six years, danger not only of physical destruction but of spiritual disintegration. Under the emergency measures which were taken to carry on the work of the University the Vice-Chancellor was entrusted with wide powers. They were used with the utmost discretion and judgment. Professor

Horton enjoyed none of the pomp and pageantry which fall to the lot of the Vice-Chancellor in normal times, but he must surely feel that the widespread and sincere tributes paid to his exceptional services afford some measure of compensation.⁷

Apart from his scientific work and his University teaching, both of which he loved, the administration work was, for him, the very pinnacle of the service he felt privileged to give his colleagues and the Institution he served. These services were so greatly appreciated within the University that he was made a co-opted member of the University Senate from 1945 to 1951 and a member of the Court from 1932 to 1951.

During the years at Royal Holloway College, possibly his most valuable service to the College was to bring about a change in attitude of the governing body towards research by urging appreciation of the idea that such activity was not only compatible with the conscientious discharge of teaching duties but was actually vital to the full development of a College of the University. A young lecturer struggling to prosecute some line of research, while shouldering a heavy load of teaching, could always count on encouragement from him, and many competent young graduates, eagerly looking forward to post-graduate research work, would never have realized their ambitions but for his efforts on their behalf.

Horton's persistence in the face of discouragement and his steady devotion to his own research, while running at the same time a department of some fifty students, with only one assistant, certainly did much to pave the way to the far happier position occupied by research in the College at the present time.

As for his private life, little was known of this side when he was in Cambridge, where, at any rate his younger contemporaries found his natural reserve and shyness difficult to pierce, although a rather withdrawn friendliness was never withheld. He married in 1911 Miss Vera Fulton in New Zealand, by whom he had a daughter, Ngaire, now married, with a son Michael Bajko. As already mentioned in this biography, Horton's second marriage was to a former student and research collaborator, Dr Ann Catherine Davies, herself a distinguished physicist and administrator; after a highly successful undergraduate career at Royal Holloway College she became a lecturer there for some seven years, dating from 1922, when Newnham College, Cambridge, wisely recognizing her outstanding qualities, welcomed her to their Fellowship. She later became Vice-Principal of the College, as well as a teaching Fellow and in 1935 was elected a University Lecturer in Physics and at this date is still actively engaged in Cambridge.

When Horton retired from his Chair at Royal Holloway College, he returned to Cambridge to live, and the dining privileges granted to him by St John's College, for which he had formed a deep affection, were a source of great satisfaction to him. Although very few of his former friends were still in Cambridge, it was a matter of very real regret to him, when in his later years, illness prevented him from continuing his regular Sunday evenings in College.

For several years after his retirement to Cambridge, his membership of Senate and Court of the University of London continued and he journeyed frequently to London for meetings, so that his withdrawal from a very active life was gradual. Until the years immediately preceding his death he had enjoyed good health and had retained a youthful vigour and vitality, enabling him to take an active interest in pursuits for which he had previously been too busy to find time. One of these pursuits was gardening, and he found great satisfaction in devoting to the cultivation of roses the same meticulous care which he had formerly devoted to the design and construction of a piece of physical apparatus.

Although in his youth, he travelled abroad a good deal he did not do so in later years. His favourite holiday resorts were Devon and Cornwall and sometimes the Isle of Man, where two of his brothers lived for some years. He enjoyed walking but had little interest in motoring, except as a means of getting from place to place.

His students at Royal Holloway College entertained for him an affectionate regard, particularly those who knew him before his administrative work in the University made such heavy demands on him; after his return to Cambridge it gave him much pleasure when, as frequently happened, former students with sons or daughters coming up to Cambridge, brought them to see him. He had a keen sense of humour and a happy knack of being able to convey volumes in an occasional concise and witty remark which delighted and often surprised those of his hearers who did not know him well and some of his 'bon mots' will long be remembered and cherished by his former students.

In writing this biographical memoir of one whom in his early years at Cambridge I, as a younger man, was privileged to know, I have been given the greatest help by Mrs Horton and am most indebted to her; I have also had my memories of the long past Cavendish days revived by correspondence with my old friend Professor John Satterly of Toronto, to whom I am also grateful.

R. WHIDDINGTON

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R. B. Jeffery

GEORGE BARKER JEFFERY

1891-1957

GEORGE BARKER JEFFERY was born on 9 May 1891. He came from a Quaker family, and remained a Quaker all his life. He was educated at Strand School, King's College, and Wilson's Grammar School, Camberwell. In 1909 he entered University College, London, to begin a course, common at that time, of two years at the college to be followed by one year's training as a teacher. He was not an entrance scholar, but his work in mathematics showed so much promise that he was elected to a scholarship in mathematics at the end of his first year.

In 1911 he entered the London Day Training College for his teacher's training. It was there that he met Elizabeth Schofield, whom he married in 1915. However he had already commenced mathematical research, and he read his first paper (1)* before the Royal Society in June 1912, the month following his twenty-first birthday. His later career showed how great an impression had been made upon him by his year's training as a teacher. However, after it was over he returned to University College as a research student and assistant to L. N. G. Filon, who was then Professor of Applied Mathematics. He always had a great admiration for Filon, though this was not uncritical, as is shown by the obituary notices which he later wrote for the Royal Society, the London Mathematical Society and the Mathematical Association.

In 1914 Filon went away on active service, and Jeffery, aged 23, was left in charge of the Department of Applied Mathematics. In 1916 he was elected a Fellow of University College. However, as a Quaker he had a conscientious objection to performing military service, so that he could not do this, nor was he allowed to remain at the college. In 1916 he spent a short time in prison as a 'conscientious objector', though later he was allowed to undertake 'work of national importance'. In 1919, when the war was over, he returned to the college, again as an assistant to Filon.

In spite of all the difficulties of the war period he had, as the list of his publications shows, maintained a steady output of original work. In 1921 he was promoted to the grade of University Reader in Mathematics.

In 1922 he became Professor of Mathematics at King's College, London, but he stayed there only two years. In 1924 M. J. M. Hill retired from the Astor Professorship of Pure Mathematics at University College, and Jeffery was appointed to succeed him. Among the assistants whom he took over from Hill was the writer of this memoir. As Filon was still Professor of Applied

* Numbers in parentheses refer to the numbered bibliography at the end of the memoir.

Mathematics, there followed a period when each of the professors of mathematics, pure and applied, had previously been an assistant in the other department.

Jeffery was elected a Fellow of the Royal Society in 1926. In the years following the war he had published a series of papers in rapid succession. But they were entirely in the field of applied mathematics, and it was here that his real interest lay, though he was always interested also in the theory of the functions which occur in the solution of problems in applied mathematics. It may be that his appointment to a chair of pure mathematics was unfortunate. He was entirely competent to act as a director of the teaching and organization of a department of pure mathematics. As his Presidential Address to the London Mathematical Society shows, he had thought deeply on the problems of teaching, and on what should be taught in such a department. However at this time graduate students were beginning to come along with problems in pure mathematics with which Jeffery was not able to deal, and he was compelled to leave much of this work to his assistants. It may also be said that though he had been very successful in the rather limited field of exact solutions of physical problems, he never advanced into the wider fields of approximation in which so much has since been achieved. He was also becoming increasingly absorbed in the problems of college and university administration. At any rate he never made any further original contribution to pure mathematics, and even in applied mathematics his original work came to an early end.

During these years he had many activities outside the work of his own department. He was Swarthmore Lecturer to the Society of Friends in 1934. In 1935 he became a member of the Senate of London University. From 1935 to 1937 he was President of the London Mathematical Society; from 1937 to 1938, President of the London Society for the Study of Religion; from 1938 to 1940 he was a Vice-President of the Royal Society. In 1939 he became Chairman of the Matriculation and School Examination Council of London University. He was President of the Mathematical Association in 1947, and in 1948 became Chairman of the South-West Middlesex Hospital Management Committee.

In World War II a section of University College moved to Bangor, Wales, and there Jeffery acted as Pro-Provost. When the war was over the College returned to London. Soon afterwards he resigned from the Astor Chair of Pure Mathematics, and became Director of the Institute of Education of London University, in succession to Sir Fred Clarke. It was a surprise appointment, since all his predecessors at the Institute had made their careers in the field of education. It turned out that it was a post for which he was admirably fitted, and it led to what was in some ways the most successful period of his life.

The Institute of Education was founded in 1932 as a successor to the London Day Training College, which had previously formed the Education Department of London University. In 1942 the 'McNair Committee' was set

up by the President of the Board of Education to consider the recruitment and training of teachers, and in 1944 it produced a report suggesting alternative schemes, but recommending in particular that the large number of colleges concerned with the work should be brought into closer association with one another and with the Universities. There were more than thirty of these colleges, many in the London area, but others scattered over the south-east of England. In 1945 the University of London accepted responsibility for the training of teachers in this area. It submitted a draft scheme as a basis for further discussion, and the task of giving this scheme a definite and practicable form therefore fell largely upon Jeffery. The elaborate shaping of the London Area Scheme was almost entirely his work, and in the reconciling of the many varied interests involved he had a long and formidable task. He had a vision of a more comprehensive Institute of Education in which all the constituent colleges and all the teachers in them should have some responsible share. In getting such a vision on to paper Jeffery was at his best. He saw the need for a definite plan to put before the Senate so that the university might know precisely to what it was committed. He produced the plan in two days of concentrated work, and it has never since needed substantial alteration.

His sense of dignity and respect for teaching forbade the casual entry of all these colleges into the ambit of the university. A full scale Inauguration Ceremony was staged. The Chancellor of the University, the Earl of Athlone, presided, and the Minister of Education and many other notables were present. Jeffery made a long speech in which he reviewed the history of all the colleges and expressed his hopes for the future of the whole organization. This must have been one of the proudest days of his life.

The Institute of Education has for a long time had, through its Colonial Department, strong overseas interests, and a specially strong body of West African students, and so Jeffery became interested in the problems of West African education. In October 1949 the Secretary of State for the Colonies invited him to visit West Africa and report upon a proposal for a West African Schools Examination Council. He went out in December 1949 and spent eight weeks in Nigeria, the Gold Coast, Sierra Leone and the Gambia. Part of the time was spent in the capital of each territory consulting the African members of legislative councils, government officials and representatives of the teaching profession. He also spent as much time as possible travelling in the Territories, meeting groups of teachers and the heads of schools. His report, dated March 1950, gives a masterly review of the whole problem. He recommended that the proposed council should have the wider title of 'The West African Examination Council', and he examined every aspect of the problem, from the types of examination which ought to be held, to such questions as whether photographs or thumb prints were better for identifying the candidates at examinations. As a result of his report the proposed Council was set up, and it has since ensured the integrity of the examinations and their validity as an index to educational standards.

The success with which he conducted his mission already described led to a still wider undertaking. The Secretary of State decided to ask Jeffery to lead a study group which visited West Africa for six months in the second half of 1951. At the same time another study group visited East and Central Africa. In September 1952 a Conference on African Education was held in Cambridge, and a report on the whole matter, *African education, a study of educational policy and practice in British tropical Africa*, was published in 1953. Presumably the report of the West Africa Study Group contained in this volume is largely Jeffery's work.

After this Jeffery paid an annual visit to West Africa to keep in touch with the work of the Examinations Council of which he was the founder. His last two visits must have given him special pleasure, for in 1956 he was accompanied by Mrs Jeffery, and in 1957, only a week or two before his death, he joined in celebrating the independence of Ghana.

He also visited Russia. In November 1955 and June 1956 groups of Soviet teachers of English and people connected with the training of teachers visited Britain as guests of the Educational Interchange Council, a small voluntary organization for the encouragement of closer relations between Britain and other countries. In return the Council was invited by the Ministry of Education for the Soviet Union to send a group to study the situation there. Jeffery was one of those selected. They visited Moscow, Leningrad and Baku, and studied schools and the training of teachers, but not university education. Some sections of the report on this visit were written by Jeffery.

He was also greatly interested in craftsmanship. He was descended from a family of wheelwrights, and was himself an expert cabinet maker. Several tables in the Staff Common Room at University College were made by him. The desk which he made for his room in college was unfortunately destroyed in the second war. Late in his life he took up silversmithing, and registered his own hall-mark with the Goldsmiths' Company. Since 1952 he had been Dean of the College of Handicraft. It was while driving home from the annual conference of this college, on 27 April 1957, that he died from a sudden seizure.

Sir Ronald Forbes Adam, in the *Bulletin of the Institute of Education* says: 'He was a man who never appeared to have too much on his hands; he could always find time to discuss a problem with anyone who called. He held decided views and expressed them openly, yet no one could quarrel with him. . . . We have all lost a dear friend, a great man and one who loved his fellow human beings.'

Jeffery was the author or part author of twenty-one original papers, besides a small book, *Relativity for physics students*.

The first group of papers, numbered (1), (2), (6), (7), (8), (9) and (10), are on special types of harmonic functions. In (1) he introduces toroidal harmonics, i.e. potential functions suitable for problems relating to two spheres, and applies them to calculate the coefficients of self and mutual

capacity for two charged conducting spheres. The analysis depends on the use of co-ordinates (u, v, w) related to Cartesian co-ordinates (x, y, z) by the formulae

$$u + iv = \log \frac{\rho + i(z+a)}{\rho + i(z-a)}, \quad w = \tan^{-1} \frac{y}{x}$$

where $\rho = \sqrt{(x^2 + y^2)}$. The two-dimensional 'bipolar co-ordinates' which he used in several later papers, defined by

$$\alpha + i\beta = \log \frac{x + i(y+a)}{x + i(y-a)}$$

are the two-dimensional form of this obtained when x/y is constant.

The other papers all centre round the celebrated general solution of Laplace's equation given by E. T. Whittaker in 1903. This consists of the formula

$$\int_0^{2\pi} f(x \cos t + y \sin t + iz, t) dt$$

where f is any function which has the period 2π in t . I remember Jeffery saying 'Anyone who can find an application of this formula will earn the eternal gratitude of Edmund Taylor Whittaker.' Actually Jeffery himself had made effective use of it. He was interested in expressing certain well-known special harmonic functions in the canonical form of Whittaker's solution. He carried out this programme for external spherical harmonics in (2), and for bipolar and toroidal harmonics in (8). In (9) he shows that Whittaker's general solution provides a ready means of expressing spherical harmonics in series of spheroidal harmonics or cylindrical harmonics; cylindrical harmonics in series of spherical harmonics or spheroidal harmonics, and spheroidal harmonics in series of spherical or cylindrical harmonics. In (10) he constructs the transformation of Whittaker's function f when the variables x, y, z are subjected to a rotation. In (7) he proves that the only cylindrical harmonics are either Bessel-function solutions, elliptic cylinder solutions, parabolic cylinder solutions, or 'rectangular harmonics' of the type $\cos mx \cos ny \exp \{z\sqrt{(m^2 + n^2)}\}$. A short note (6) investigates the form of lines of force and equipotential lines at a point of equilibrium in two-dimensional fields or fields with axial symmetry.

The next group of papers, numbered (3), (4), (5), (15), (16) and (21) is on the theory of viscous flow. The key to these researches is given at the beginning of (5), where he says 'The object of this paper is to search for some *exact* solutions of the equations of motion of a viscous fluid.' His purpose was to obtain exact numerical solutions of the Navier-Stokes equation, a purpose which was admirably fulfilled by his discovery of a number of new and

interesting types of flow. The first paper (3) gives the earliest expression of the Navier-Stokes equation in general orthogonal curvilinear co-ordinates. This was followed in (4) by a study of the steady rotation of a solid of revolution in a viscous fluid, the inertial terms being neglected. Bipolar co-ordinates appear again in this paper in the discussion of the problem of a viscous fluid generated by the rotation of any two spheres with different angular velocities about their common diameter.

Lest it should be thought that these researches are quite remote from the field of work of the writer of this memoir, it may be pointed out that one of the problems discussed in (4) leads to the pair of integral equations

$$\int_0^\infty f(k) J_1(k\tilde{\omega}) dk = \omega_0 \tilde{\omega} \quad (\tilde{\omega} < c),$$

$$\int_0^\infty k f(k) J_1(k\tilde{\omega}) dk = 0 \quad (\tilde{\omega} > c).$$

Jeffery says 'It has not been found possible to solve these integral equations directly'; but by an indirect method he obtained the solution

$$f(k) = \frac{4\omega_0}{\pi} \frac{\sin ck - ck \cos ck}{k^2}$$

Actually these 'dual integral equations' are of the type which I have studied in my book *Fourier integrals* §11.16, and the solution found by Jeffery may be obtained by the method given there.

The search for exact solutions of the full equations was resumed in (5), and the following important types of flow were discovered as examples of flows in which either the stream lines or lines of constant vorticity are identical with one family of the orthogonal curvilinear co-ordinates:

- (a) the motion around a canal in the form of a circular arc;
- (b) the motion between rotating circular cylinders with a given normal flow across the surfaces;
- (c) the flow between two infinite planes inclined at an angle.

The last type of flow is specially interesting as providing an exact solution of a 'boundary layer' problem.

Paper (15) discusses the motion produced in a viscous fluid by the rotation of two circular cylinders, inertial terms being neglected, so that an analogue of Stokes's paradox appears. Naturally bipolar co-ordinates are used again in this paper. In paper (16), which is also concerned with the 'creeping motion' at small Reynolds numbers, he considers the motion of ellipsoidal particles immersed in a viscous fluid. The corresponding problem involving spherical particles had been treated by Einstein, who gave a simple formula for the increase in the viscosity of the plasma due to the presence of the

particles. It was shown that, if the particles are ellipsoidal, the increase of viscosity could still be represented by a formula of Einstein's type with a modified numerical factor.

In the last paper on the subject (21), written in collaboration with Miss M. Stimson, the authors study (again by means of bipolar co-ordinates) the motion of two spheres in a viscous fluid at small Reynolds numbers.

Jeffery wrote one paper, (11), on elasticity. He was rightly impressed by the importance of constructing a corpus of exact solutions of problems in elasticity as well as in fluid dynamics, and in this paper he gave the complete solution in bipolar co-ordinates of problems of 'plane stress' and 'plane strain'. These solutions throw considerable light on two-dimensional problems of elasticity, and in some cases are immediately applicable to such practical matters as the stresses produced by the driving home of hot plastic rivets.

There followed a group of papers, (12), (13), (14), (17), (19), (20), on the theory of relativity. Jeffery's predilection for exact solutions of limited problems is also shown in his researches on Einstein's general theory of relativity. In two papers (13) and (14) he discussed the field of a point electron and of gravitating particles. Paper (17) gives an elementary proof of the vanishing divergence of the Einstein tensor

$$G_{\mu\nu} - \frac{1}{2} g_{\mu\nu} G.$$

Two papers, written in collaboration with O. R. Baldwin, discuss the propagation of gravitational waves of small amplitude, and the possible circular orbits of an electron around a fixed nucleus.

The paper on statistics, written in collaboration with E. M. Elderton and Karl Pearson, depends on manipulations of Bessel functions, and presumably this was Jeffery's contribution to the paper.

Jeffery's point of view in all these researches was extremely practical. He was looking all the time for exact solutions of definite physical problems, and often gives at the end of his papers a little table of numerical results. One can also see in all his work the influence of Filon. Stress and strain, and the theory of viscous flow, were Filon's subjects too. In Jeffery's obituary notice of Filon, he says 'Filon attained a mastery in the art of manipulating Legendre and Bessel functions and the like—the ideal equipment for research in classical applied mathematics at that stage.' This also might be applied to Jeffery.

Jeffery's Presidential Address to the London Mathematical Society (1937) is entitled 'Mathematical studies in the modern universities'. One of his themes is his dislike of the scholarship system and the premature specialization which it causes. He says 'it may succeed in sorting out the boys with the best mathematical ability, but it does so at a tremendous cost. The last two years of a boy's time at school are years of rich opportunity in which many subjects can be treated much more broadly and freely than they can be at an

earlier stage. If he hopes for a mathematical scholarship those invaluable years must be devoted almost entirely to mathematics, and most of us would probably agree, not to the best sort of mathematics. Few headmasters are prepared to accept this monstrous situation without some effort to mitigate its evil. . . .’ Further, university mathematics is so different from school mathematics that a test based on over-specialization in school mathematics will not necessarily pick out those who are really suited to a mathematical career.

The problem of what should be taught in the university also interests him. He is concerned that mathematical examination papers should be more than mere cross-word puzzles. The teacher should strive to attain a broad view of his subject, and to discern the main roads of its development right down to the new work that is being carried on today. ‘I have no hesitation in proposing as a principle of selection that he should choose those main roads which lead right into the work of the greatest living mathematicians.’ He then shows how these considerations apply to the practical planning of a mathematical course in a modern university.

The lecture also contains the suggestion that mathematicians should hold an annual conference of a few days’ duration, meeting each year at a different University. This proposal has since been carried into effect.

In conclusion I wish to thank Professor W. R. Dean, Professor G. Temple, F.R.S., and Professor E. H. Warmington for their help in the composition of this memoir.

E. C. TITCHMARSH

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E. L. Kennaway.

ERNEST LAURENCE KENNAWAY

1881-1958

THE PERSONAL RECORD which the subject of this memoir deposited with the Royal Society consisted of little more than a brief statement of his qualifications, appointments, and distinctions, and an incomplete list of publications, such as might have been submitted as part of an application for a post. This is characteristic of the self-effacement which marked the whole career of a very remarkable man. Few knew him well; those who did felt a great affection for him. For some brief information about his childhood and youth I am indebted to his sister, Mrs Ethel Kay, and his fellow-undergraduate, Dr H. C. Squires. His major scientific contributions have had a decisive and far-reaching influence on the history of cancer research. During much of his life he was handicapped by ill-health. In fact, nearly 30 years ago, his then chief had already written him off, and was casting around for a possible successor, at a time when Kennaway's most important work was only beginning. Few can have achieved so much under such adverse conditions.

Kennaway was born in Exeter on 23 May 1881, the son of Laurence James Kennaway. His grandfather, William Kennaway, who was twice Mayor of Exeter, played a leading part in combating the cholera epidemic in that city in 1832. William Kennaway sent all his four sons and two of his five daughters to New Zealand, beginning in 1851. In 1869 the two sisters sailed for home in a ship which met with an unknown fate; the youngest son, Charles, was washed overboard and drowned in a voyage from New Zealand in 1872; some details of this voyage are given by Sir Ernest in a short article published in *St Bartholomew's Hospital Journal* in 1947.

As a boy aged 8 or 9 years, Kennaway was kept away for a time from his private school because of delicate health and was encouraged to spend much of his time in the open. It was at that early age that he first became interested in natural history and began to develop the acute sense of perception and the powers of detailed observation that remained with him throughout his life. In an article written only 7 years ago ('Consciousness and evolution in plants and insects'), he described himself as an amateur naturalist, and his various newspaper articles on birds and wild life mentioned in the bibliography bear testimony to the validity of this claim. His sister has allowed me to see a notebook which he kept in 1889-91. It is a remarkable piece of work for a boy not yet 10 years of age, as the following extracts show:

'27 November, 1889. Saw 3 swallows (*hirund* and *rustica*) flying southwards over Homefield*, Exeter. I have not seen in any book any record of such late stragglers. I noticed that their rumps were not white and

* Homefield was the family home where Kennaway spent his childhood. It is now known as Lazenby, and is one of the annexes to a hall of residence of the University of Exeter.

that their tails were widely forked, so that they could not have been house-martins.

'We have pigeons, mostly tumblers. They are fairly good breeders, though one pair (called "Solomon" and "Nell") whenever they get any young turn them out of the cot on to the roof of the house. These we have to rear by hand which is very troublesome, as it takes at least 3 people to feed one. One person to hold it still, another to open its bill, and another to push food down its throat. One pair never have more than 2 young birds at a time. And now [4 April 1891] we have four young ones. Only one pair can fly yet, though the others will in a week or so. The father bird guards the pair which can fly, sitting by them and continually giving a kind of grunt; each grunt being accompanied by a jerk of the whole body. All these four are white and (as the young ones always have) have very long soft bills. The birds seem always hungry and directly we go into the outhouse where their food is kept, down they all come with a tremendous clatter of wings. If the food is thrown out before they fly down, the draught from their wings drives the seed all over the yard. Several have been killed by cats, and some were stolen but only one died from an illness. They are mostly fed on Poultry Rice, or a kind of mixed grain. When it rains or hails they fly down to the yard, and stretch themselves on the ground in the most amusing attitudes, so as to get wet. In the terrific snowstorm of the 9th of March, 1891, the careful father "blunderbus" kept two young birds alive, though the biting East Wind blew the snow straight into the cot.'

There are daily entries for the period 22 February to 18 March 1891 (the year of the great blizzard). They include the following:

'Feb. 25. Wednesday. Very thick fog. After, fine. Went through Gay's orchard and the lower copse. Saw a lapwing flying heavily over the woods, the first I have seen for over a month. On getting near the woods, we startled a pair of ring-doves which were feeding on the ground: later on we heard some bullfinches and saw a few robins and fieldfares. Also saw some catkins and some rose-bay which is rather rare. In the woods we saw some old nests of magpies, jays, and ring-doves. Saw a bullfinch.'

'March 9. Mor. Bitterly cold. Aft. and Eve. snowing hard. Wind E. Therm. Fahr. 4 deg. below freezing point.

'March 10. Mor. Snow 5 ins., deep and snowing hard. Found an icicle on one of the outer window sashes 6 ins. long. A drift of snow outside one of the windows (facing W. and N.W.) was 1 foot deep. The wind was E. It was most amusing to see the house-sparrows perch on a slanting roof covered with snow; they fell on their sides, kicked, struggled and at last shot down into the roof gutter. The ice in our tub was about half an inch thick this morning. Thermom. Fahr. between 5 and 6 below freezing point. The drifts along the Mount Pleasant Rd. were between 14 and 15 feet high. The wind (which was east) was bitterly cold, was

full of sleet and sometimes bits of broken bits of ice, and it was impossible to walk or see in it.'

'March 16. Mor. White frost. The barometer has sunk so low that a friend of ours who has several years in New Zealand said that if it had been there, he would say there is going to be a earthquake. After. Thawing like mad, so that the grass in our field (which is between 2 and 3 inches high) was pressed down flat onto the ground with the moisture like a wet cloth.'

Kennaway's school studies in Exeter were supplemented by private tuition. Not long before his death, he wrote to me in the following terms about A. W. Clayden, a former head of the institution which is now the University of Exeter: 'In the nineties he was my first instructor in Chemistry and in his favourite subjects of physiography and geology, of which he was an absolute born teacher; I have never enjoyed any teaching more than his. After my father's death in 1904, we left Exeter, and I never met him again.'

Of his childhood in Exeter and the social conditions at that time, and of his bare subsistence as a student in London, Kennaway has himself written, in his article 'The biographical aspect of social medicine' (1951-52).

In 1898 Kennaway became a student at University College London, and in the following year he went to New College, Oxford, with an open scholarship in natural science. He obtained his Bachelor of Arts degree in 1903, with a first class in the final honours school in physiology. His clinical studies were completed at the Middlesex Hospital, where he held a University Scholarship in 1904, and he qualified in medicine in 1907. He was elected to a Hulme studentship at Brasenose College, Oxford, in 1909 and in 1910 he held a Radcliffe Travelling Fellowship of the University of Oxford. He obtained his D.M. degree at Oxford in 1911 and the London D.Sc. degree in Physiological Chemistry in 1915.

During his long working life Kennaway held relatively few appointments. He was Demonstrator of Physiology at Guy's Hospital from 1909 till 1914, and Chemical Pathologist at the Bland-Sutton Institute of the Middlesex Hospital from 1914 till 1921. In the latter year he became Chemical Pathologist in the Research Institute of the Cancer Hospital (Free) and in 1931, following the death of Professor Archibald Leitch, he was appointed Director of the Research Institute (now the Chester Beatty Research Institute of the Institute of Cancer Research: Royal Cancer Hospital) and Professor of Experimental Pathology in the University of London. In 1946 he retired from this dual office and was offered hospitality in the pathological laboratories of St Bartholomew's Hospital, where he worked until his death on New Year's Day, 1958. On his retirement from the chair, London University conferred on him the title of Professor Emeritus.

In recognition of his outstanding scientific achievements, Kennaway received many honours and distinctions. He was elected a Fellow of the Royal Society in 1934, and was awarded a Royal Medal of the Society in

1941. He was created a Knight Bachelor in 1947. In 1937 he became a Fellow of the Royal College of Physicians and was awarded the Baly Medal. His other distinctions included: the William Julius Mickle Fellowship of the University of London (1922); Honorary Fellowships of New College, Oxford (1942), the New York Academy of Medicine, and the Royal Society of Medicine; Honorary Membership of the American Association for Cancer Research (1947), Honorary Foreign Membership of the Royal Academy of Medicine of Belgium (1954) and Honorary Associateship of the Rationalist Press Association (1949); the Walker Prize of the Royal College of Surgeons, the Garton Medal and Prize of the British Empire Cancer Campaign (1946), the Anna Fuller Prize (with others) (1939), and the Osler Memorial Medal (1950).

In 1920, Kennaway married Nina Marion Derry, of Edgbaston, Birmingham, who survives him. There were no children. Many tributes have been paid already to the part which Lady Kennaway played in her husband's life and work. He himself made acknowledgement with characteristic succinctness when he received a presentation on the occasion of his retirement from the Directorship of the Chester Beatty Research Institute: 'She has been a voluntary worker here, in every sense of the word, for 24 years, and has given me immense help in both biological and statistical matters. All this has been done very unobtrusively, and I am deeply grateful to her.' As a scientific assistant and collaborator she was tireless, patient and meticulously careful. She never obtruded on his administrative functions and she was a charming hostess in the hospitality which he freely dispensed. In his latter years she gave him immense personal help and succour without which his disabilities might well have made him physically helpless.

In February 1949, *The Lancet* published an anonymous article on Parkinson's disease. Kennaway is now known to have been the author. In it, he says that he first began to notice the symptoms in himself about 20 years earlier. It was, in fact, in 1929 that I first met him and by that time the tremor of the hands and the characteristic gait were easily recognized. His article gives a detailed account of the symptoms and the disabilities and the treatment which he used to overcome the latter. He also describes the disadvantages of the mixture of drugs which he found most helpful and the measures to be taken to counteract these adverse effects. His acknowledgement to 'those enigmatic plants of the Solanaceae which accomplish, within the microscopic compass of their cells, organic syntheses, doubtless of value to themselves, which elsewhere would require the resources of a research institute', is symptomatic of the respect which he always had for the mysteries of chemical synthesis. Swimming he found much easier than walking, as well as more pleasurable, and it is probable that the exercise which he obtained in this way did much to maintain him in otherwise good health and to preserve his mobility. When, some months before his retirement, he met with a serious street accident and was confined to bed for a long period, his friends feared that he would find it difficult or impossible to regain his powers of

walking. But they took too little account of his fortitude and his dogged determination.

The intense driving force of his will-power must have contributed in large measure to his mastery of the increasingly adverse circumstances of his latter years. These included a series of operations for various conditions, including an operation for cataract which all but robbed him of his sight. He assured me, in one of his confiding moments, that it was very salutary to have the lenses of one's eyes removed—it enabled one to view other people in a way that was far from flattering to them! During a visit to Finland he was laid low with a devastating attack of asthma. Shortly after his return, when he was revived miraculously by ACTH treatment, he spoke with obvious relish of the unceremonious way in which he had been bundled on to a plane for England during an interlude between attacks 'because they were scared that I should die in their hands'. The cool detached way in which he spoke about such occurrences gave one the impression that he was conscious of living on borrowed time.

Kennaway was very punctilious in his observance of the social and other conventions. Up to a point he had great respect for authority and was intensely loyal to the institutions which he served. When he was in a position of authority himself he expected, and usually received, the same measure of respect and loyal support. He did not encourage undue familiarity, perhaps because of his inherent shyness and dislike of emotion, and few people were tempted to take liberties with him. But he was a delightful and entertaining companion when he was 'off duty' and allowed himself to relax. He enjoyed his position as a leader and was particularly kind and generous to his younger colleagues. He took a great delight in their progress and their successes and encouraged them by his infectious enthusiasm and his unbounded confidence in their ability to overcome the difficulties of their tasks. He was completely intolerant of careless or slovenly work and set very high standards of scientific and intellectual integrity. He had a logical and orderly mind, backed up by almost encyclopaedic knowledge. This is shown very clearly in his publications covering a wide variety of subjects, which also reveal his powers of observation with minute attention to detail, to which allusion has already been made.

His high sense of duty, his devotion to ideals and principles and his championship of his younger colleagues and of the under-dog sometimes led him into violent conflict with others. On such occasions he was no respecter of persons. His sardonic and sometimes caustic humour, his intense earnestness, coupled with the symptoms of his Parkinsonism which he strove manfully to overcome made him a formidable and indeed devastating adversary. I have seen quite exalted persons wilt under the lash of his tongue.

During the period when I was his senior assistant I was taken aback one day by being asked by Kennaway to entertain at lunch some American visitors, whom he had invited to the Institute. He explained that he no longer had any status as he had submitted his resignation as Director. What

had happened was that the Chairman of the Hospital Committee, a distinguished Field Marshal, had dismissed a member of the technical staff of the Institute because of an alleged dereliction of duty in the care of the animal house. Kennaway took exception to this on the grounds (a) that the action was unjust, and (b) that the Chairman had usurped his authority. I was present at a subsequent stormy interview between Kennaway and the President of the Hospital who had been peremptorily summoned back at the beginning of a week-end in the country. He was completely innocent in the matter and seemed very surprised at the violence of the tornado which struck him. In the sequel, Kennaway was reinstated in his office as was his technician and it was the Field Marshal who resigned. But Kennaway was not always so successful in imposing his will and he had grievances against a number of people who had thwarted his plans or who had declined to co-operate. He was inclined to interpret such action as motivated by personal hostility towards him and to attribute sinister designs where none existed.

The circumstances surrounding the incident of the Field Marshal left their mark, and when the Hospital was offered the gift of the building which has become the Chester Beatty Research Institute, Kennaway saw in this a plot to have him removed from the immediate vicinity of the hospital and its clinical departments. He refused to take any part in the planning of the reconditioning of the building, and the installation of laboratory furnishings, and it was left to others to perform these tasks. But he took a very active personal share in the protection of the building against air raids during the war. I think, from remarks that he once made to me, that he regretted having resigned from the Biochemical Society, of which he had been an Original Member, on account of some dispute about publication.

During the years at the Royal Cancer Hospital, Kennaway took no part in public discussion and his collaborators were deputed to present the results of joint researches at scientific meetings and to speak for the Institute on other occasions. While this was due in part, no doubt, to the handicap of his disability, it was probably also due to his natural reticence and shyness. In latter years he was more forthcoming and frequently gave addresses and talks and participated actively in meetings. His interjections were usually pithy and apt and often couched in scornful terms. He was not active in political affairs. If he had been, he would have been an ardent reformer. In political complexion he was inclined towards the left. He was full of bitterness towards the Munich settlement of 1938, and full of sympathy for the Czech victims of that settlement. He was also violently opposed to anti-semitism in whatever form. He had intense feeling about the sufferings undergone by old horses. The bibliography refers to two letters which he published in *The Times* in 1951 expressing concern about some features of the export of horses, and in a pamphlet on 'The problem of the old horse' which was apparently printed privately, he gives the outlines of a scheme to reduce the sufferings of old horses. Characteristically, the subject is introduced with quotations from *The Golden Ass* of Apuleius. In his last publication before his death (*Brit.*

Med. J., 1957, 2, 1485), he commented on nursing from the patient's point of view.

Kennaway was out of sympathy with orthodox religious beliefs. He found it difficult to reconcile their doctrines with reason, and described himself as a rationalist (see letter published in *Rationalist Review* for October 1954). He said in his speech in acknowledgement of the presentation made at his retirement that he was not a believer in any form of personal immortality. 'The real human immortality consists, to my mind, not in any survival of the mere individual, but in the continuity of human effort in great endeavours.' In the one book which he published (*Some religious illusions in art, literature and experience*: London, 1953), and also in shorter articles, he has expressed his scepticism about various aspects of religious teaching.

Those who were privileged to be Kennaway's colleagues in the Research Institute of the Royal Cancer Hospital will always associate the laboratories there with his presence—a tall figure in a long white coat moving around, body tilted slightly forward, with a quick shuffling walk; or, seated in his office with his trembling hands supported on the arms of his chair, frequently changing his spectacles (I think he carried three pairs), and often removing his coat and putting it on again or opening and closing the window. He seemed to have a hypersensitivity to changes of temperature and other atmospheric conditions. Or perhaps one would be collaborating with him in preparing the draft of a paper—an excellent training in patience; he refers in his article on Parkinsonism to the extreme slowness in handwriting, 'embarrassing when another person is waiting for what one writes'. This would occasionally be interrupted by reference to one of the numerous manuscript notebooks on his bookshelf—some of them documented; some of them indexed; all of them models of careful recording. Each morning there would be the daily routine of 'painting his mice'. In the afternoon he would join the other members of the staff to tea in the library, sometimes joining with them in animated conversation; sometimes listening; at other times sitting absorbed and apparently oblivious of others until stung to a pungent rejoinder by some comment with which he disagreed. His team formed a friendly and dedicated community, somewhat heterogeneous in character, but closely knit by his stimulating leadership.

Scientific Investigations

Although Kennaway's earlier work was concerned mainly with problems in a number of general fields of physiological chemistry, some of them in conjunction with such well-known scientists as Cathcart, Leathes, Kossel, C. H. Browning, J. S. and J. B. S. Haldane and others, most of his work, over a period of more than 35 years, dealt with cancer research either directly or indirectly. It was in cancer research that he made his reputation, and it is for his outstanding contributions in this field that he will be chiefly

remembered. He was essentially an experimentalist, but he never forgot that the main aim of experimental cancer research is to assist in combating a disease which takes a heavy and increasing toll of human life.

Side by side with his experimental studies he carried out a series of investigations, mainly with the collaboration of Lady Kennaway, and mostly based on analyses of death certificates, of the incidence of cancer in various organs in relation to occupation, environment, race and social class. This type of work gave full scope for his flair for amassing, coding, tabulating and classifying data. For the most part, both in these and in his experimental studies, he was content to ascertain and to record the facts. Interpretation and speculation he preferred largely to leave to others. The 1931 paper on the incidence of cancer of the bladder and prostate drew attention to certain occupations which seemed to be especially associated with an enhanced liability to cancer of the bladder. The 1936 paper on the incidence of cancer of the lung and larynx emphasized the steep rise in the numbers of deaths from cancer of the lung in England and Wales during the years 1921 to 1932, especially in males, and must have had considerable influence in stimulating the widespread interest which this problem has attracted since. The 1947 paper completed the analysis of death certificates up to 1938 and fully confirmed the trends revealed by the earlier study on cancer of the lung and larynx.

Arsenical cancer has long been known as a hazard associated with the prolonged use of arsenic for treatment of various skin diseases and, more recently, as an industrial disease associated with the handling of arsenical sheep dips. Kennaway always had in mind the possibility that arsenic was an aetiological factor of more widespread importance than these limited relationships suggest. As long ago as 1922 he described, in collaboration with Leitch, the experimental production of cancer by arsenic, but this work has never been followed up, and in later years he himself seemed to set little store by it. But it doubtless inspired his later study (with Barry and Bunbury) of the effect of arsenic on oxidation-reduction systems, and the possibility that arsenic might be a causative agent in lung cancer led him and his collaborators to make determinations of the arsenic content of tobacco, tobacco smoke and cigarettes (1950, 1957), and of the solid matter of town air (1952). However, in scientific matters he never allowed prejudice to distort his judgement of the evidence and in his article 'A contribution to the mythology of cancer research' (1942) he scornfully demolished loose statements that had been made in the literature about arsenic as a causative agent of cancer in various species of animal.

Kennaway's most significant achievement, with which his name will always be associated, was the discovery of the carcinogenic chemical agents and especially those of the polycyclic aromatic hydrocarbon group. The detailed biological effects of these compounds are recorded in the series of papers on 'The production of cancer by pure hydrocarbons' published in the *Proceedings of the Royal Society* (1932-1942). He himself was always the first to

acknowledge the essential and complementary part played by his associates (Mayneord, Hieger, Hewett and the writer and others, with his technical assistant, F. Goulden), but the initiative was entirely due to Kennaway, and the rapid development of this field of investigation in the 1930's was the direct outcome of the painstaking, methodical and often disheartening work which he carried out, unaided except by his technical assistant, during the previous decade. Throughout the later development of the work the biological testing of the synthetic chemical compounds was carried out almost entirely by him alone, except that he received much assistance from his wife in the *post mortem* examination of the experimental animals.

The discovery of these carcinogenic chemical agents opened up an entirely new chapter of cancer research. It demonstrated for the first time that malignant tumours exactly akin to human cancer could be induced by the application to animals of pure chemical compounds of known molecular structure, and it lent support to the view that much, if not most, human cancer is due to the action on the cells of the body of chemical agents, both extrinsic and intrinsic. This view, however, still lacks proof. One very interesting observation which Kennaway made (with Burrows and Hieger, 1932) was the finding that the same compound which on application to the skin produced squamous celled carcinomas gave on subcutaneous injection spindle celled sarcomas. But the need for caution in interpretation of experiments in this field was emphasized by the further finding of the same authors (1936) that under certain circumstances fatty media alone could induce in rats tumours of low malignancy. Biological investigation of carcinogenic agents has been extended in many laboratories and they have been found capable of inducing malignant tumours in a wide variety of tissues and species, and the tumours conform to a range of varying histological types. The availability of such agents was followed by their extensive use by cancer research workers in every country and there is now a vast literature on the subject. There are now, of course, many other types of carcinogenic chemical agent than those originally produced in the research laboratories of the Royal Cancer Hospital, supplemented later by extensive work in the same field by L. F. Fieser and his collaborators at Harvard.

The fascinating story of the discovery of the carcinogenic polycyclic hydrocarbons is now well known to those who are interested in cancer research, but some of the main outlines may be recalled. Kennaway's starting point was the demonstration by Yamagiwa and Ichikawa (1915) that malignant tumours of the skin could be induced in rabbits by the application of coal tar and by Tsutsui (1918) that mouse skin reacted similarly. Moreover, following the classic observations of Percivall Pott on chimney sweeps' cancer in the eighteenth century, it had become recognized that industrial skin cancer was associated with exposure to such materials as coal tar, shale oil and mineral lubricating oils. Kennaway set himself the task of investigating the nature of these materials to determine whether their cancer-producing action was due to specific ingredients and if so, to identify them. He elected

to work on coal tar, and in his first papers, published in 1924, he reviewed the various forms of tar in relation to their carcinogenic potency, eliminated some of the fractions and many of the known constituents, and on the basis of both industrial and experimental evidence concluded that high temperature coal tar produced far more cases of industrial cancer than all the other materials together, and that the active component was present in the highest boiling fractions of the tar. He concluded: 'It seems not unlikely that the cancer-producing substance is some compound as yet unknown, which is unstable and present in amounts as small as those of the hormones in the body-fluids, or the vitamins in foods. As in the case of some hormones, its identification may be long delayed even when very concentrated preparations can be obtained.'

The next stage was an attempt to simplify the problem by using artificial tars of (presumably) simpler composition. In Berthelot's classic work on the pyrolysis of acetylene to give benzene, much tar is formed and Kennaway adapted Berthelot's technique to give a tar which proved highly carcinogenic. Following Staudinger, he prepared tars by pyrolyzing isoprene at 700 and 820 °C, and these also proved carcinogenic, that formed at the higher temperature particularly so. This work pointed strongly to a hydrocarbon as the active constituent of the tar. Carcinogenic tars were similarly obtained by pyrolysis of yeast, human skin and a non-carcinogenic Californian petroleum. This work was published in 1925. Later, a very potent carcinogenic material was found in a high-boiling fraction of the mixture formed, following the procedure of Schroeter, by treatment of tetrahydronaphthalene with aluminium chloride at 37 °C for 18 hours. Thus, for the first time, a highly carcinogenic mixture was formed by a non-pyrolytic process. This work was published by Kennaway in 1930. In the meantime, Mayneord had found, in 1927, that this synthetic carcinogenic mixture gave a well-defined fluorescence spectrum. In further studies, in which he was joined by Hieger, Mayneord found that the same bands were present in the fluorescence spectra of most of Kennaway's carcinogenic tars, although less clearly defined. If the substance responsible for these fluorescence bands were also the carcinogenic constituent, then this was obviously an invaluable clue. Further work proceeded on the assumption that the two properties were, in fact, related.

At this stage I was instrumental in providing Hieger with a number of specimens of fluorescent anthracene hydrocarbons, among them a sample of 1 : 2-benzanthracene which had been prepared by E. de Barry Barnett. This was found to have a fluorescence spectrum of a type indistinguishable from that of the mixture prepared from tetrahydronaphthalene, but displaced towards the ultra-violet region. By another fortunate coincidence, the following year (1929) saw the publication of two chemical papers (by Clar and by Fieser & Dietz, respectively) in which were described the synthesis of a number of polycyclic hydrocarbons related to 1 : 2-benzanthracene. Samples of these were prepared by Goulden in Kennaway's laboratory and

one of them (designated 1 : 2 : 7 : 8-dibenzanthracene) gave malignant tumours of the skin of mice. Two others (1 : 2 : 5 : 6-dibenzanthracene and its 3'-methyl derivative) gave papillomas (Kennaway 1930; Kennaway & Hieger 1930). In 1929, I had accepted Kennaway's invitation to join forces with him, and I was able to show that the supposed 1 : 2 : 7 : 8-dibenzanthracene was, in fact, the 1 : 2 : 5 : 6- compound. Clar, and following him, Fieser & Dietz, had failed to appreciate that a molecular rearrangement had taken place in the reaction, which was expected to give 1 : 2 : 7 : 8-dibenzanthracene.

In this way the first potent carcinogenic hydrocarbon, 1 : 2 : 5 : 6-dibenzanthracene, was found. But this was not the compound responsible for the characteristic fluorescence spectrum studied by Mayneord and Hieger, nor was it known to be a constituent of coal tar or other carcinogenic tars. So the original problem was still unsolved.

How it was solved, some 2 or 3 years later, has been related by Kennaway in an article published in the *British Medical Journal* in 1955. Hence, details need not be given here. By the fractionation of 2 tons of coal tar pitch, Hieger obtained a crystalline material from which Hewett and I were able to isolate a pure highly carcinogenic hydrocarbon and to establish by synthesis that it was the previously unknown 3 : 4-benzpyrene. Its solutions had intense fluorescence and the spectrum was identical with that of the carcinogenic fraction prepared from tetrahydronaphthalene. This fraction probably contains 3 : 4-benzpyrene, although this has never been formally proved. Kennaway took great satisfaction in this successful completion of the task to which he had devoted himself so methodically during a period of years and which had seemed to many to be a wild goose chase. The third way in which the clue of the fluorescence spectrum was followed up led to the synthesis of a large number of carcinogenic hydrocarbons related to 1 : 2-benzanthracene and some analogous hydrocarbons, and from these studies some broad generalizations were elaborated as to the structural conditions favourable for carcinogenic activity within this group.

In the course of his studies on artificial carcinogenic tars, Kennaway had found that such a tar could be produced by pyrolysis of cholesterol (Kennaway & Sampson 1928). From what is now known about the chemistry of cholesterol it is doubtful whether this has any significance in relation to the aetiology of cancer, yet Kennaway was always interested in this remarkable constituent of animal cells. In 1930, at his instigation, I initiated some experiments on the Diels hydrocarbons which it yields on dehydrogenation with selenium and it was this beginning that led us, four years later, to methylcholanthrene as a potent carcinogenic hydrocarbon of the benzanthracene group, related to the sterols and bile acids. Sir Ernest was greatly impressed by the later work of Hieger on the weakly carcinogenic action of samples of cholesterol of varying degrees of purity, and he devoted a good deal of time in his latter years to stressing his view of the importance of these

findings of Hieger. Probably the final chapter on the significance of cholesterol in relation to cancer has still to be written. Kennaway's 'hunch' that this is an important field of investigation may yet be vindicated.

J. W. COOK

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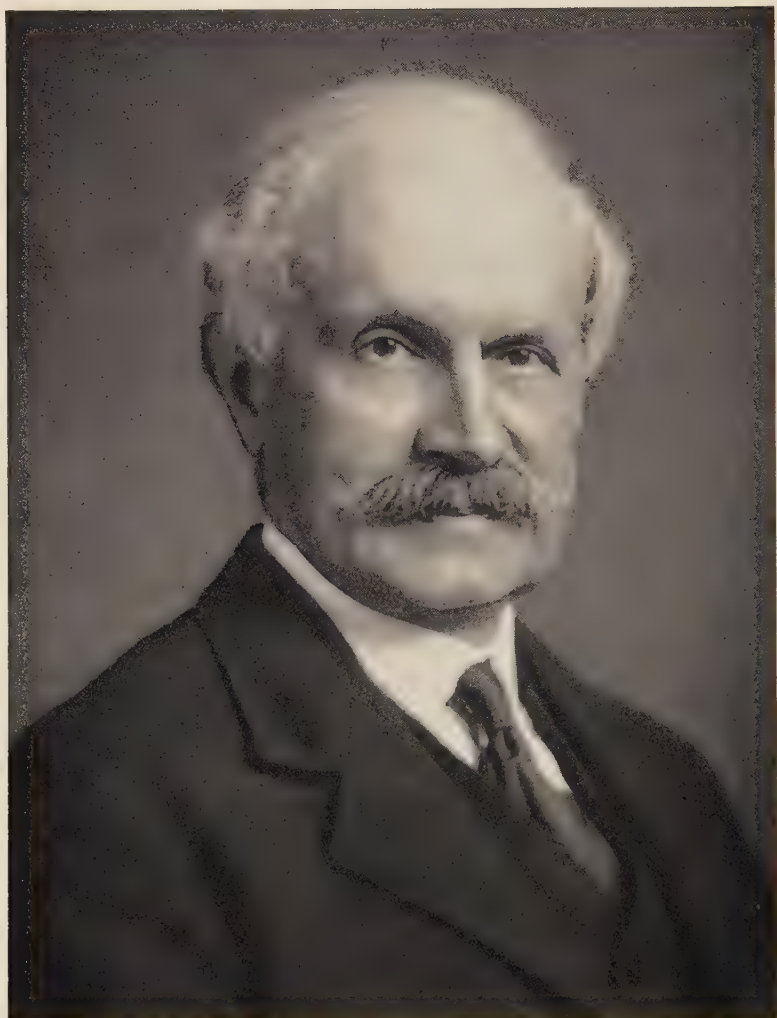
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J. Graham Kern.

JOHN GRAHAM KERR

1869-1957

JOHN GRAHAM KERR was born on 18 September 1869, at Arkley, Herts, the son of James Kerr, M.A., a former Principal of Hoogly and Hindu College, Calcutta, and of Sybella Graham, of Hollows, Dumfriesshire. He was third in a family of four with three sisters. His father was a well-known educationalist and the author of various works dealing with a number of topics including Indian educational problems, English orthography and aspects of human nature, and Thomas Carlyle.

Graham Kerr, as he was known throughout the greater part of his life, lost his mother in early childhood and grew up under the influence of his father who although his tastes were mainly literary had a broad interest in general science, especially in natural history and evolution, in which he was widely read. His father superintended the early stages of his education, including latin and mathematics, and encouraged the reading of such books as Darwin's *The voyage of the Beagle*, Waterton's *Wanderings*, Wallace's *Amazon and Malay Archipelago*, etc. In addition, his library contained a large selection of classical works, especially poetry and history, and Graham Kerr was brought up in a general atmosphere of literary culture. His schooling began at the parish school of Dalkeith, Midlothian, under William Young, a good example of the old-fashioned type of parish schoolmaster who did not hesitate to give special time and attention to any boy who in his opinion possessed the natural capacity to benefit by his teaching. After a short time at the Collegiate School, Edinburgh, he passed on to the Royal High School, where he was specially influenced by Munn, the mathematics master under whose tuition he became Dux of the Fifth Form. He subsequently enrolled in the University of Edinburgh and first concentrated on higher mathematics and natural philosophy. He then studied geology, botany and zoology and finally decided to follow out the curriculum in medicine. This was interrupted when on a wintry afternoon in February 1889, this young medical student of nineteen, returning home from his classes picked up a copy of *Nature* at the book-stall in Waverley Station, and read an announcement which in his own words 'determined the whole future of my life'.

This was a notice by P. L. Sclater, Secretary of the Zoological Society of London, stating that Captain Juan Page of the Argentine Navy, was undertaking a new expedition for the survey of the Pilcomayo from the Paraná to the frontiers of Bolivia, and was prepared to give a place on his staff to any qualified person who might wish to avail himself of this excellent opportunity of exploring a most interesting country.

Graham Kerr had been an enthusiastic reader of books on exploration and natural history and this notice seemed to offer a chance of passing from the lecture room and laboratory, haunts of the past three years, into the realms of romance. He was selected for this opening and sailed for Buenos Aires on 2 June 1889. The account of this famous expedition (1889-91), *A naturalist in the Gran Chaco*, was not published until 1950, by which time the events to which it referred had become mainly of historical interest. During the expedition he was engaged in the study of general natural history and especially ornithology, and many new species were collected, but unfortunately as the result of an unforeseen calamity most of his collections were lost. Among the relics which survived he was especially interested in the drawing of a man made by a Redskin Indian companion belonging to the Natokoi tribe, who had no understanding of pictorial art. When it was explained, however, the meaning of pictures was soon grasped and the drawing produced by this Indian is of importance as being the first ever made by this particular race. Two points of interest in this drawing are the accuracy in the number of fingers and toes and the relative unimportance—as expressed by size of drawing—of the head as compared with the genitalia and hands and feet. Graham Kerr's field notes on the habits of a wide variety of animals including the adaptations of those living in swamps, protective colouration, and his observations on the Natokoi showed even at this early age that he was not only an observer and naturalist of exceptional ability, but also a man of resource, courage and endurance above the ordinary. The incredible adventures in which he was involved during the course of this ill-fated expedition to the Pilcomayo are graphically described in his book, and show the part he played in averting disaster.

He returned to England in 1891 and entered Christ's College, Cambridge, as a scholar. He was a brilliant student and obtained first-class honours in Part II of the Natural Sciences Tripos. By this time he had already published work on the avifauna of the Pilcomayo and on the anatomy of the pearly nautilus. Whilst a student he was a keen member of the Cambridge University Cruising Club and together with W. B. Hardy and H. H. Brindley took part in a famous cruise across the North Sea through the dykes of Holland, ending at Borkum. He was also a member of the Cambridge University Natural Science Club and Secretary of the Shakespeare Society, and took part in many other university activities. At the same time he was making preparations for a second expedition to Paraguay with the main object of studying and collecting *Lepidosiren*, as he had already decided to dedicate his research career to the embryology and life history of the Dipnoi. On this expedition (1896-97) he was accompanied by J. S. Budgett of Trinity College, Cambridge, and the material they collected formed the basis for an extensive series of investigations on the morphology and especially the embryology of *Lepidosiren paradoxa*. These collections and also those of three subsequent expeditions to the Chaco region are preserved in the Department of Zoology in the University of Glasgow.

On his return Graham Kerr was appointed demonstrator in animal morphology in the University of Cambridge and also elected a Fellow of Christ's College and awarded the Walsingham Gold Medal in 1898. Four years later he was appointed Regius Professor of Natural History in the University of Glasgow, where he remained until 1935. The Chair of Natural History, at that time, included not only zoology and mineralogy, for which it was founded in 1807, but also geology and palaeontology, taught after 1876 under a lectureship. The previous occupants of the Chair as a rule had been more interested in palaeontology and mineralogy and a reorganization of the department was long overdue. The independence of geology was recognized in 1903 by dividing the old Chair of Natural History into distinct Chairs of Zoology and Geology. Graham Kerr thus became the first Regius Professor of Zoology, a change of title which he strongly resented as he would have preferred to retain the original form which is still maintained in the other three Scottish universities.

Throughout his professorship he was especially interested in the teaching of medical students and his lectures were famous. Generations of students throughout the world, many of whom have become eminent in their professions, still remember his lucid and stimulating delivery, enlivened with interesting anecdotes and illustrated by excellent diagrams which were drawn on the blackboard so rapidly and with such precision. Unkind gossip said that at least part of their accuracy was the result of inconspicuous guiding marks placed on the board the previous night. He took immense pains in the preparation of these lectures and they were being continually revised and brought up to date throughout the whole of his professorship. He was of the opinion that the zoology course for medical students should instil interest in the subject, train powers of observation, and by the study of lower animals lay the foundations of human anatomy. Parasitology was recognized by the inclusion of several of the more important animal parasites, not only in the lecture course but also in the laboratory. His approach to the subject was largely morphological and embryological and is embodied in his textbook *Zoology for medical students* (1921). Apart from his heavy teaching and administrative duties, Graham Kerr carried on his research work on various aspects of dipnoan embryology, and a whole series of papers was published by himself or his colleagues. He was quick to discover outstanding abilities and gave every assistance possible to the younger members of his staff and by his personal influence helped to develop research interests different from his own. On the other hand he did not lightly encourage impecunious students to enter into such a speculative career as zoology in the early days of this century.

In the department, when not lecturing or demonstrating, he used to work behind a locked door, and at a knock this barrier would be opened only a few inches and a somewhat ferocious face would appear in the chink to see who was there. If admitted, the visitor would be welcomed with a charming old-world courtesy which all his friends will remember with affection.

Graham Kerr was a man of very vigorous personality and aroused strong opposition in certain individuals. With his keen wit and skilled oratory he was a formidable antagonist in debate and on one occasion his opponent, a distinguished surgeon, was so enraged that he drew himself up and said 'We would like to have it out with rapiers', to receive the crushing retort 'Septic no doubt!' One of his greatest achievements was to create a department of zoology in the University second to none in its housing and equipment. When Graham Kerr first went to Glasgow the department occupied a few dingy rooms and whilst the Hunterian Museum provided a certain amount of anatomical material, the collections had received such little care that boxes containing over 200 insect type specimens of J. C. Fabricius were even found in a rubbish heap. After years of endeavour he succeeded in obtaining the erection of a monumental new building containing a departmental museum. The main laboratory, designed to provide accommodation for 150 students at a time, had a large north window reaching the full height of the building so as to enable students in the back rows to use daylight for their microscopes. It is sad to record that in later days another building was erected on the field in front of this window blocking out the light and defeating the object of this large expanse of glass. The museum was provided with a honeycomb roof to avoid the entrance of direct sunlight with its well-known deleterious effect on museum specimens. The exhibits as much as possible were arranged in cases with a matt-black background and indirect lighting which could be turned on and off by the observer. A handprinting press was obtained and all the labels were printed in gold leaf on matt black paper in order to avoid the inartistic and distracting effect of white labels and in conformity with Graham Kerr's opinion that one's first attention should be directed to the object and only subsequently to the label. He had a keen artistic appreciation of display technique and under his direction the zoological museum became one of the finest in the country. He was successful in obtaining many notable additions to the Museum's contents including the Bishop collection of Coleoptera together with funds for the employment of an entomologist. He was justly proud of this museum with its beautiful exhibits and never tired of showing visitors some of its treasures. One of his favourites was the poison fang of a gigantic, extinct, Opisthoglyphous snake, *Bothrodon priddii*, obtained among a mass of bones in the Chaco by a missionary friend, Andrew Pride. Unfortunately no other part of the skeleton has ever been found but comparing the size of this fang with that of a modern poisonous snake, it is estimated that this monster may well have been some 60 feet long, surely one of the most formidable animals that ever lived.

Graham Kerr took a very active part in University affairs and was a member of the University Court from 1913-21. In addition, he served on the governing bodies of the Royal Technical College, the Andersonian College of Medicine and the Royal Infirmary. He was particularly interested in the development of marine biology and was mainly responsible for the founda-

tion of the temporary marine station at Rothesay. He was President of the Scottish Marine Biological Association (1942-49) and previously had devoted much time to the organization of the Millport Station. Mainly through his efforts, a building fund was established which enabled a new wing to be added to the existing premises which was opened at the beginning of the Second World War. Outside his department Graham Kerr took a very active part in the development of general scientific activities and especially natural history in Scotland. He was elected a Fellow of The Royal Society in 1909, served on its Council 1920-22, and 1936-38, and was a Vice-President 1937-39. He was elected a Fellow of the Royal Society of Edinburgh in 1903 and served as a Councillor for three terms and as a Vice-President from 1928 to 1931; he was awarded the Neill Prize of this Society in 1904. He was President of the Royal Physical Society of Edinburgh, 1906-1909, and also of the Royal Philosophical Society of Glasgow, 1925-1928. He was knighted in 1939 and other recognitions included the Honorary LL.D. of Edinburgh (1935), and St Andrews (1950), an Honorary Fellowship of Christ's College, Cambridge (1935), the Linnean Gold Medal (1955), and Associate Membership of the Royal Academy of Belgium (1946). He served for many years on the Council of the British Association and was President of Section D (Zoology) at the Oxford meeting in 1926.

Graham Kerr had a high sense of public duty and his presidential addresses both on that occasion and to the Royal Philosophical Society of Glasgow dealt with biology and the training of the citizen. The titles of other addresses, —'Biology and work,' 'Biology and the State', 'the cultural value of biology', 'Science and Government'—all indicate his interest in the wider implications of a biological training.

Fisheries constituted another subject in which he took an active part and he was a member of the Advisory Committee on Fishery Research from its beginning in 1919, and Chairman from 1942-1949.

One subject in which he took a very special interest was the application of correct biological principles in working out a system of camouflage. He had noticed at the opening of the Kiel Canal as far back as 1895, that France and Germany had already adopted means to diminish the conspicuousness of their warships, whilst British ships were still painted black with yellow funnels. On the outbreak of the First World War he wrote a letter to the Admiralty in September 1914, advocating the use of obliterative shading and disruption to render our ships less conspicuous. The suggestion was adopted in theory but the work was left to the discretion of individual commanding officers who in most instances were quite ignorant of the principles involved. Eventually a special department was formed to deal with naval camouflage under expert supervision, and from 1917 onwards to the end of the War more than five thousand ships were treated in this way. He rendered a great public service in the part he played in getting a system of disruptive colouration adopted and this method of protection was adopted almost universally during the Second World War, even for land installations.

Graham Kerr held his appointment at Glasgow University under the old regulations with no retiring age, but in 1935 he offered his resignation from the Chair. Various reasons influenced this decision and the death of his wife in 1934 was doubtless a contributing factor. He had just been elected Member of Parliament for the Scottish Universities and wished to devote more of his time to political affairs. He hoped that he might be able to play a useful part in the House of Commons as an exponent of the scientific point of view. He was a very regular attender, served on various committees and for a time was Chairman of the Parliamentary Science Committee. In addition he often expressed the view that a complete change of occupation and environment gave one a new lease of life. He left Glasgow and moved to a delightful country house, Dalny Veed, at Barley, near Royston, Herts, where he was to spend the last 22 years of a long life. He was a Member of Parliament from 1935 until 1950, when University seats were abolished and during these years completed the account of his two famous expeditions to the Gran Chaco (1889-91 and 1896-97). This book has been described as the best account dealing with South America since Charles Darwin's *Voyage of the Beagle*. Towards the end he became increasingly frail and for the last two years of his life was completely blind, an affliction which he accepted characteristically saying that he had seen remarkably well for more than eighty years and could hardly complain. He never lost his mental faculties, however, and up to the last took a lively interest in anything concerning natural history or any of his old students. I saw him the evening before he died and even then, although very frail, he was mentally alert, and still showed his wise understanding and gentle humour.

He was the last survivor of the famous zoologists of the nineteenth century, for the most part widely travelled, good naturalists, with an almost encyclopaedic knowledge of their subject. He had a very original mind and could enliven any discussion with amusing anecdotes and shrewd comments. His outlook on zoological problems was essentially morphological and phylogenetic and he tended to mistrust the experimental approach which grew up especially after the First World War. He was an enthusiastic admirer of Charles Darwin's work and in particular his belief in the value of field work in contrast to laboratory experiment. His output of zoological work was very considerable, especially in his early days, but latterly his many public duties restricted his scientific activities.

He married his cousin, Elizabeth Mary, daughter of Thomas Kerr, W.S., in 1903, and Mrs Graham Kerr died in 1934. In 1936 he married Isobel, daughter of A. Dunn Macindoe and widow of Alan E. Clapperton, LL.D. He is survived by Lady Graham Kerr and by two sons of the first marriage.

RESEARCHES

Graham Kerr, like many zoologists, started by collecting various animals and plants—butterflies, algae, diatoms, fossils, and so on—and then went through

a period when his main interest was in osteology. His more serious work was inaugurated by Professor Adam Sedgwick who gave him a number of specimens of *Nautilus* for a study of their anatomy. These animals had been kept in sawdust moistened with rum so were in no condition for the investigation of histological detail, but the gross anatomy was sufficiently well preserved. The results of this research besides adding new anatomical details threw light on the general morphology of the genital ducts in Mollusca and also on the sucker-bearing arms of the Cephalopoda (Siphonopoda). This knowledge was of value in assessing the relations of the Cephalopoda to other sub-divisions of the Mollusca and emphasized the importance of concentrating special attention upon the more archaic members of any phylum, and comparison with the features of other members of the phylum. He was also strongly of the opinion that no general theory, nor any reputedly primitive type, should be regarded as sacrosanct and immune from further examination.

This approach was followed in his later work on archaic vertebrates and led him to throw overboard the generally accepted view that the legs of land vertebrates had evolved out of the paired fins of fishes. He also abandoned the idea that *Amphioxus* and the *Cyclostomata* are in their general structure to be regarded as primitive, rather than highly specialized in relation to their peculiar habits.

The direction of Graham Kerr's post-graduate research work was determined mainly by the general atmosphere of the Cambridge School of Zoology which at that time, under the inspiration of Francis Balfour, continued to be morphological. There was then a very imperfect knowledge of some groups of the lower vertebrates especially of certain Ganoids and the Dipnoi. Accordingly he decided to dedicate his main researches to the study of these groups and especially *Polypterus*, *Lepidosiren* and *Protopterus*. His investigations have provided a fairly detailed account of the embryology of *Lepidosiren* and *Polypterus*, until then completely unknown, and also of *Protopterus* which was also unknown, except for a short study by Budgett before his material was handed over to Graham Kerr for completion. During these investigations a survey was made of other groups of aquatic vertebrates with the object of determining the validity or otherwise of the generally accepted views as to the evolution of vertebrates. Some of his main conclusions are summarized as follows:

Skin and its derivatives

The cement organs familiar in the ectoderm of young amphibians and in *Lepidosiren* and *Protopterus* develop in *Polypterus* as endodermal outgrowths from the gut and their presence in the ectoderm is a secondary transference. The indications are that they are organs of great antiquity in the history of vertebrates. The main pigmentation of the skin in *Lepidosiren* is by wandering parenchyma cells, sensitive to light and forming melanin granules in response to its influence, creeping helio-tropically towards the light by pseudopodial movement and colonizing the epidermis. The marked colour changes in

response to varying conditions of daylight are due to expansion or retraction of the pseudopodia, rather than to streaming of pigment granules within the cell.

In the case of the nervous system his more important conclusions were (1) that the familiar and easily observed appearance of the nerve trunk growing peripherally from the central nervous system and joining up secondarily with its end organ, either motor or sensory, is deceptive and that the nerve trunk is simply a highly differentiated bridge which is already continuous between the central nervous system and the end organ at a stage before these have come to recede from one another with the increase in size of the developing body; and (2) that in the fully developed *Lepidosiren* the apparent break of continuity at the synapse is also misleading as fine neurofibrils can be traced across it. In the retina of *Lepidosiren*, owing to the large size of the cell elements, the early stages in the development of the rods are clearly visible.

A detailed study of the dermal or external gills of *Lepidosiren*, *Protopterus* and Urodele amphibians showed so deep-seated a correspondence that he was led to abandon the old view which considered these organs as merely recent developments and to come to the conclusion that they are very ancient characteristics of vertebrates. Their presence in primitive vertebrates together with the fact that they are equipped with muscles for the production of movement, would make them ideal ancestors for the limbs—paired fins or legs—of more recent vertebrates. Evidence in support of this view was developed in various publications dealing with the evolution of limbs.

Enteron

The pharyngeal outgrowth present in all Gnathostomata above the elasmobranchs and which in land vertebrates becomes the lung, was examined in detail in the case of *Lepidosiren* and *Polypterus*, and led him to the unexpected conclusion that the air bladder of the modern teleost represents not an ancestral phase of the typical lung, but the modified right lung of an evolutionary stage common to all vertebrates above the primitive elasmobranchs. As a result of his work on *Polypterus* the cement organs seen in the young stages of amphibia were considered to have probably originated as enteric outgrowths and later on become transferred to the skin.

Coelomic organs

The embryology and comparative anatomy of the renal organs formed a very important part of his researches on archaic vertebrates. *Polypterus* is of special interest in this connexion as it showed the history of the nephrocoeles, both as regards their origin and their subsequent development. The evidence of *Lepidosiren* and *Polypterus* is also of interest in the evolution of the curious male genital duct of teleosts. The conversion of myotomes into muscle fibres was traced out in detail in *Lepidosiren*, but the detailed study of the skeleton and blood systems was carried out mainly by his colleagues.

Vertebrata

Graham Kerr summarized his views concerning the various sub-sections of the phylum Vertebrata as follows:

(1) In *Amphioxus* and cyclostomes particular care should be taken not to allow the undoubtedly primitive nature of some of the details of their development to obscure the fact that their organization as a whole is of a highly adaptive nature in correlation with their peculiar mode of life. As an example the supposedly ancient lamprey *Palaeospondylus* is probably to be interpreted—in accordance with the appearance of macerated *Lepidosiren* larvae—not as a cyclostome but as an archaic type of gnathostome at about the same evolutionary level as the Dipnoi.

(2) A consideration of the methods of movement of vertebrates suggests that lateral flexure is the more primitive method, as indicated by the universal presence of longitudinal muscles subdivided into serial blocks (myotomes) in early stages of development. This has been supplemented later by propulsion by means of muscular projections of the body-wall (limbs). The simple styliform limb diverged along two lines, one leading to the development of paired fins, adapted to a swimming mode of life, and the other to the development of jointed limbs adapted to movement on land.

Mollusca

Graham Kerr's work on Mollusca was mainly concerned with *Nautilus* and *Spirula*. His work on *Nautilus* gave for the first time an account of the morphology of this archaic type based on the investigation of a considerable number of specimens. The most important results furnished an explanation of (1) the arrangement of the coelom and reproductive organs; (2) the morphology of the nervous system; (3) the morphological nature of the arms of the Cephalopoda—not pedal as generally believed, but merely out-growths from the head region; and (4) the phylogenetic relationships of the Cephalopoda to other molluscs and especially the Amphineura.

His work on *Spirula* was also based on several specimens and resulted in amplifying knowledge of its general structure. Special attention was devoted to the alimentary canal and its appendages and to the very complicated reproductive organs of the male. The probable evolutionary history of the shell in the Cephalopoda is discussed in relation to that of *Spirula* where the shell acts as a float. Reasons are given for considering the straight shell of, for example, *Orthoceras*, as not primitive but secondary, and the straightness of the last-formed part of the shell in *Spirula* suggests that it is in the process of evolving in the direction of straightness. The cephalopod shell is regarded as evolving through the following sequence: (1) external and protective; (2) chambered and acting as a float, while still external and protective, as in *Nautilus*; (3) internal and rotated within the body so as to become endogastric in position, as in *Spirula*; (4) developing a dense rostrum

to act as a counterpoise to the elongation of the body, as in *Spirulirostra* and *Belemnites*; (5) the disappearance of the air chambers and the internal shell functioning solely as a support, as in *Sepiolo* and *Teuthids*; and (6) the complete disappearance of the shell, as in many octopods.

EDWARD HINDLE

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Irving Langmuir

IRVING LANGMUIR

1881-1957

IN the winter of 1942, as retiring President of the American Association for the Advancement of Science, IRVING LANGMUIR addressed the membership on the topic 'Science, commonsense and decency'. He was concerned with the interaction of scientific progress and human endeavour and took the opportunity to reveal his own ideas concerning problems of human behaviour and morality. He recognized two categories of phenomena in modern physics which he termed convergent and divergent phenomena. He included among the convergent phenomena all those fluctuating details of individual atoms and molecules which average out giving a result that converges to a definite state. Many of the phenomena of classical physics belong in the convergent category. To a second class he assigned the divergent phenomena where, from a small beginning, increasingly large effects are produced. Quantum physics includes many such phenomena. Langmuir chose as an illustrative example the Wilson cloud-chamber experiment. Here, a single swift particle, say an alpha particle, whose release from its parent is unpredictable as to time and direction, subject only to the laws of chance, in its passage through the chamber leaves a trail of ions. If the chamber atmosphere be super-saturated, droplets are formed; illumination renders these visible and photography can record the multiple divergent consequences from a single event, the release of the swift particle. In further development of this theme of divergence Langmuir went on to state that 'when we consider the nature of human affairs it is to me obvious that divergent phenomena frequently play a role of vital importance'. Langmuir's birth on 31 January 1881 comes within the category of his divergent phenomena, from which beginning profound effects on the development of physico-chemical science finally resulted.

He was born in Brooklyn, N.Y., and attended the public schools of that city save for an interval of three years during which the family moved to Paris where his father was in charge of the New York Life Insurance agency. There, at a small boarding school in the suburbs of Paris, he was, as an American and through the good offices of a friend, 'freed from much of the absurdly rigorous discipline to which the French boys were subjected. Thus I could spend time alone in the school laboratory and was encouraged by one of the teachers to learn the use of logarithms and solve problems in trigonometry, subjects not required by the curriculum.' In the same record of his youth he revealed that 'my real interest in science was derived from my brother Arthur, who encouraged me to have a workshop at the age of nine,

and later a laboratory when I was only twelve. Until I was fourteen I always hated school and did poorly at it.' After the years in Paris Langmuir went to school at the Chestnut Hill Academy in Philadelphia, Pa., and finally at the Pratt Institute High School in Brooklyn, N.Y.

From high school he entered the School of Mines of Columbia University and graduated with the degree of Metallurgical Engineer in 1903. He proceeded immediately to Germany and engaged in graduate studies in physical chemistry at the University of Göttingen where he received the Ph.D. degree in 1906. The dissertation, carried out at the suggestion of and directed by Professor W. Nernst, was concerned with the dissociation of water vapour and carbon dioxide around glowing platinum wires. On the assumption that, in a rapid stream of gas, there would be only very slight recombination of the dissociated products during the short time required for diffusion from the hot wires to the cold layers a short distance away, Langmuir made measurements with wire temperatures from 1351 to 1537 °K. He deduced thereby values for the dissociation of both gases from 1000 to 3000 °K obtaining results somewhat less than those obtained by Nernst and von Wartenberg using other techniques. This dissertation foreshadowed the initial activities in the General Electric Research Laboratory at Schenectady some years later. After Göttingen he became an instructor in chemistry at the Stevens Institute of Technology in Hoboken, N.J., across the river from and looking towards the island of Manhattan and New York City. From that laboratory in 1908 he analyzed mathematically the velocity of reactions in gases moving through heated vessels, dealing with the effects of convection and diffusion. These were shown to act in a way equivalent to a decrease in the velocity of reaction. A summer employment by the Research Laboratory of the General Electric Company, Schenectady, began an association with W. R. Whitney, the Director of the Laboratory, and with W. D. Coolidge, with Hull and with Dushman which helped to make the G.E. Research Laboratory at once one of the most respected sources of basic science and one of the most fruitful centres of applied science. The success of the transition from the university environment at Stevens to the industrial research laboratory at Schenectady was due in large measure to the genius of Whitney as the director. Of him Karl T. Compton wrote that 'he has largely set the pattern and philosophy of the modern industrial research laboratories, one of the unique achievements of this century'. Whitney demanded, of those he chose, an '*optimistic activity*' which 'should be placed first among the requisites. It is placed above *knowledge* because, without it, little that is new will ever be done except by accident. With active optimism, even in the absence of more than average knowledge, useful discoveries are almost sure to be made.' Langmuir brought both optimism and activity to the Research Laboratory in the summer of 1909. When the summer was over, there was on the one hand an invitation to stay and on the other 'a world of things to interest him there'. The university lost a potential professor of high quality but basic science, through Whitney's 'judgment of men and . . . his generosity in dealing with them',

found a lifelong servant, enjoying, through Whitney, 'every possible encouragement and support' and with the fullest 'freedom to explore the unknown'.

The first phase of Langmuir's activity in Schenectady was concerned with the thermal conduction and convection in gases at high temperatures, metal filaments, notably the tungsten filaments recently developed by Coolidge, providing the high temperature sources. The behaviour of hydrogen in contact with heated filaments was startling, revealing an abnormal heat loss from the filaments, the more pronounced the higher the filament temperature. Langmuir, in 1912, with the experience of his dissociation measurements on water vapour and carbon dioxide in mind, boldly attributed the heat loss to 'a chemically active modification of hydrogen' which he correctly ascribed to dissociation of the molecules into atoms. The disappearance of hydrogen from a lamp was identified with the adsorption of atoms on the low-temperature cooled glass walls of the lamp the atoms recombining to form hydrogen molecules when the vessel walls were raised in temperature. From these researches came early values for the heat of formation of hydrogen molecules. In 1913, there followed the studies of 'clean-up' of oxygen and nitrogen in a tungsten lamp. At the time of their publication the writer was a research student with Bodenstein in Hannover. As the only English-speaking student in the laboratory he was assigned the task of reporting on these papers to Bodenstein's colloquium. The problem immediately arose as to the translation of the Americanism 'clean-up'. To the amusement of the audience, the term '*die Verschwindung*' was employed probably because the genders were troublesome to visiting Englishmen and because German nouns ending in -ung were feminine, thus permitting an easy decision as to the correct form of the definite article. Later, it emerged that '*die Verschwindung*' has an eerie, ghostly overtone. '*Das Verschwinden*', equally unequivocal as to gender, was said to represent a better translation of 'clean-up'. The reactions of hydrogen, oxygen and nitrogen with heated tungsten filaments are classical studies of reactions between gases at very low pressures and solid surfaces. They revealed the basic chemistry of hydrogen dissociation, the extraordinary strong bonds between adsorbed oxygen and a tungsten surface, far exceeding in magnitude the bond energies between tungsten and oxygen in tungstic oxide, the formation of WN_2 from molecular nitrogen and each atom of tungsten that evaporated, the catalytic action of water vapour in carrying relatively enormous amounts of tungsten from the hot filaments to the cold walls resulting in blackening of the tungsten lamps, atomic hydrogen playing an essential part in the process.

In the case of each gas studied, with the exception of the special case of water vapour, Langmuir showed that the blackening of a lamp was due solely to evaporation. He could 'conclude with certainty that the life of the lamp would not be appreciably improved even if we could produce a perfect vacuum'. The basic studies on rate of evaporation in high vacua gave way to studies of the rate in inert gases at different pressures. Nitrogen at

atmospheric pressure was found to reduce remarkably the rate of evaporation. Heat losses from the filaments in nitrogen decreased the lamp efficiency. A study of heat loss from small wires in gas led to the unexpected discovery that heat loss is relatively independent of filament diameter whereas the light radiated is proportional to the surface and, hence, the diameter. These were the basic studies out of which the nitrogen-filled lamp and, later, the argon-filled lamp became available. A coiled helix of fine diameter wire became the source of high efficiency gas-filled lamps, in all except the smallest wattages.

From 1913 to 1916 the published researches are concerned with such matters and their by-products. These include measurements of the vapour pressure and melting point of tungsten, further refinements of the atomic hydrogen studies, vapour pressures of platinum and molybdenum, and condensation pumps with an improved form of vacuum pump, a high vacuum mercury vapour pump of extreme speed.

In late 1916 and in 1917 there began Langmuir's studies of the constitution and fundamental properties of solids and liquids. He was greatly stimulated by the results of the X-ray analysis of solids. His own studies of the strong forces operative at solid surfaces led him to an examination of the properties of both solids and liquids in terms of chemical forces and more particularly the properties of liquid surfaces.

In extension of the earlier work of Rayleigh, Pockels, Devaux and Marcelin on the behaviour of oil films on water surfaces, Langmuir made a revolutionary development by measuring the spreading of *pure* substances on water surfaces. He examined the extension of controlled amounts of organic molecules containing polar groups such as —COOH , —CO— or —OH groups and demonstrated that such molecules became oriented in the water surface with the hydrophilic polar group in the water layer and the hydrophobic hydrocarbon portion of the molecule extending upwards from the surface. The area occupied per molecule could be calculated from the molecular weight of the substance and the Avogadro number and from the area to which the oil film must be compressed before the surface tension changes abruptly. In this way it was demonstrated that a series of saturated fatty acids such as palmitic and stearic acids had identical areas per molecule and that the length of the hydrocarbon chain increased by about $1.2 \text{ \AA}$ per CH_2 increment. Palmitic acid, $\text{C}_{15}\text{H}_{31}\text{COOH}$, for example, had a length of $19.6 \text{ \AA}$ while stearic acid, $\text{C}_{17}\text{H}_{35}\text{COOH}$, had a length of $21.8 \text{ \AA}$, each having a cross section of $24 \text{ \AA}^2$. These data of Langmuir provided a fascinating independent determination of the shapes and sizes of molecules. An unsaturated molecule such as oleic acid, $\text{C}_{17}\text{H}_{33}\text{COOH}$, containing a double bond towards the middle of the hydrocarbon chain, was found to have a cross section twice the area of the saturated stearic acid. Langmuir concluded that the double bond became anchored in the water layer by reason of its strong affinity for water. The great solubility of ethylene in water compared to the saturated hydrocarbon ethane is evidence for this. Later, Rideal was able to show that, under compression, the double bond could be forced out

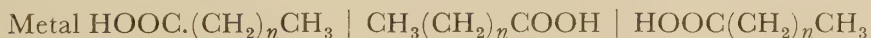
of the water layer so that it could no longer be oxidized by permanganate in the aqueous solution in contrast to the ease of oxidation of uncompressed oleic acid films with the same reagent.

The Langmuir trough and balance became an instrument of enormous utility in the determination of shapes and sizes of molecules, in the decision between alternative proposed configurations for complex molecules, in the demonstration of gaseous, liquid, liquid-expanded and solid films on surfaces. It is to Langmuir that we owe the decisive recognition of the unimolecularity of such films, the concept of the monolayer. Extension of these studies revealed that the gaseous films had the properties of a two-dimensional gas with an ideal equation $FA = kT$ per molecule, with F the spreading force and A the area, analogous to the ideal equation of state for the three-dimensional gas. Deviations from ideal behaviour were expressible by a van der Waals type equation. A series of fatty acids at a given temperature revealed a transition from two dimensional gaseous to condensed phase analogous to the gas-liquid transitions for gases.

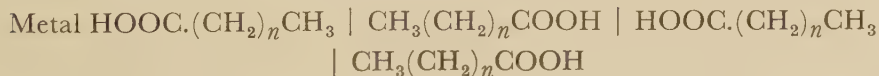
Two aspects of the film studies call especially for mention, the one the dedicated collaborator in all the work, Katherine B. Blodgett, and the beautiful simplicity of the techniques which, together, they employed. A silk thread, suitably water-proofed, resting on the surface of the water constituted an ideal barrier between film and water surface. The film could be placed under a constant pressure, P dynes per centimetre, by placing outside the barrier an excess of 'cylinder oil'. Langmuir and Blodgett together revealed how stearic acid monolayers under such compression could be built up into oriented layers on a metal surface by the simple device of dipping the metal through the film and then withdrawing it again. In this way the first dip and withdrawal gave one layer,



Each successive trip added two layers as indicated thus:



A simple test reveals the correctness of the orientation. The first, third, fifth, etc., layers are hydrophobic, with thier surfaces of CH_3 groups. If, after an immersion, the metal were withdrawn, not through the film but through clean water, the number of layers would be even and the surface wet by water, hydrophilic, as indicated by the external COOH group:



A 'cylinder oil' film could likewise become a constant pressure container for a monolayer of any material with a larger spreading pressure than that of the cylinder oil. Whether the monolayer was solid or liquid could then be

revealed by another drop of cylinder oil placed at the centre of the monolayer. If this were a liquid layer the cylinder oil would spread circularly, if solid the monolayer would tear. Both liquid and solid monolayers were thus simply revealed. Monolayers of proteins were studied by such techniques and also in the form of built-up layers. The viscosity of the monolayers was also studied as an index of the state of the monolayer, whether solid or liquid. The chemistry of monolayers, so excellently developed later by Rideal, was employed by Langmuir to differentiate between various configurations of sterols.

The transition from the consideration of monolayers on water to monolayers on solid surfaces was a natural development and led to Langmuir's concept of adsorption of gases on solid surfaces as a time lag between condensation and evaporation of the impinging gas molecules. The adsorption isotherm for the fraction θ of a uniform surface occupied at a given gas pressure, p , became

$$\theta = \frac{bp}{1+bp} \quad (1)$$

where b represented the ratio of the rates of condensation and evaporation at a given temperature. This equation which has passed into the literature of adsorption assumes monolayer adsorption and one molecule occupying each elementary space in a uniform solid surface. It represented only one case in many considered by Langmuir who indicated the modifications which would be necessary for multi-layer adsorption, non-uniform surfaces and dissociative adsorption. Langmuir's treatment derived largely from his concept of adsorption in terms of chemical forces though his initial measurements of adsorption of gases on plane surfaces of glass and mica are excellent examples of what would now be termed physical or van de Waals adsorption. It was more than a decade later that the fact of multilayer adsorption, the possibility of both physical and chemisorption of a given gas at a given surface in different regions of temperature and the consequences of heterogeneity in the surface of the adsorbent became the objectives of special experimental and theoretical study. Langmuir's basic approaches to the problem of adsorption of gases have been a common element in all the newer work.

Langmuir wove his ideas on monolayer adsorption at surfaces into a general theory of reactions at surfaces in communications to a famous *General Discussion* of the Faraday Society in 1921. On the new view, chemical reaction occurred between adjacently adsorbed reacting species on the given surface. This view superseded the earlier ideas stemming from the work of Noyes, Whitney and Nernst that reactions of solids with a liquid phase were determined in velocity by a stationary layer of product adjacent to the solid surface through which the reactant in the liquid phase must diffuse in order to reach the solid surface at which the chemical process would occur instantaneously. Such rate processes were essentially diffusion processes as Noyes and Whitney, and later Nernst, demonstrated by decisive experimentation.

Bodenstein adopted this concept to interpret his work with Stock on the decomposition of stibine and his work with Fink on the kinetics of the contact sulphuric acid process. The rate of the stibine decomposition was proportional to the 0.6 power of the hydride. The retarding influence of sulphur trioxide in the contact reaction between sulphur dioxide and oxygen on platinum, which was inversely proportional to the 0.5 power of SO_3 led Bodenstein to assume that the rates were determined by diffusion of reactants through an atmosphere of condensed gas covering the reaction surface. Because the diffusion of SO_2 would be slower than that of oxygen Bodenstein accounted plausibly for one form of the observed kinetics.

$$+\frac{d[\text{SO}_3]}{dt} = k \frac{[\text{SO}_2]}{[\text{SO}_3]^{1/2}}$$

The condensed layer on the surface in this case was sulphur trioxide, the thickness of the layer being proportional to the square root of its pressure.

Langmuir, reasoning from his concept of chemisorption as due to short range chemical forces, rejected this explanation. He showed that Fink's data on the adsorption of sulphur trioxide were not compatible with the concept of a thick condensed layer. On the contrary he assumed that chemical reaction occurred in the monolayer adjacent to the solid surface at rates governed by the fractions of the surface occupied by the reactants and products. Thus, in the generalized process



where C , the product, was markedly adsorbed, a kinetic expression of the form

$$\text{rate} = k \frac{[A][B]}{[C]} \quad (2)$$

might be expected. In the case of sulphur dioxide oxidation, using equation (1)

$$\theta_{\text{SO}_3} = \theta_C = \frac{b_c p_c}{1 + b_c p_c} \propto C^{\frac{1}{2}} \quad (3)$$

In this manner Langmuir reviewed not only his own experimental data but also the earlier data on rate processes in gaseous systems at solid surfaces. The ideas set forth in 1921 became the *leit-motif* for much of the kinetic research of the 1920's by Rideal, Hinshelwood and others. It was built into the textbook treatment of chemical kinetics notably by Hinshelwood in England, Schwab in Germany and with especial applicability to contact catalysis by Rideal and Taylor. The Langmuir theory remains the basic approach to the problem of surface kinetics. Rideal and Eley later emphasized a variant of the Langmuir concept of reaction between adjacently adsorbed species. They showed that some kinetic data could be interpreted as reaction

between an impinging molecule with a reactant adsorbed on the surface. It is a tribute to Langmuir that, after 40 years of intensive review and test, his concept of reaction kinetics at surfaces remains unchallenged and intact in all its essentials.

Langmuir's foresight, in the formulation of his reaction rate theory, to cover all the several possibilities, can be illustrated by one further example. It is implicit in the Langmuir treatment, and was explicitly so stated by him, that the rate theory is applicable only when the processes occurring between adjacently adsorbed species are rate determining. This excludes from his method of treatment all processes which are now termed 'diffusion-controlled', where the rates of transport to and from the surface are the operative factors. This is true in many reactions in liquid systems and even in many gaseous reactions. A further, rate-controlling process which might modify Langmuir kinetics, not appreciated in 1921, was the slow chemisorption of one of the reactants. This appears to be an important feature of another industrial process, the synthesis of ammonia from nitrogen and hydrogen. Here, the slow sorption of nitrogen has been claimed as the slow step in the sequence of reactions, slower than the association of H atoms with N atoms and NH and NH₂ radicals on the surface. Here again, most recent studies reveal the definite possibility that the reactions of adjacent adsorbed species may still control.

In the spring of 1919 the Research Laboratory at Schenectady was in a ferment energized by Langmuir's theory of valence, an octet theory, 'essentially an extension of G. N. Lewis's theory of the "cubical atom"'. Langmuir pointed out that 'the structure of atoms has been attacked mainly by physicists who have given little consideration to the chemical properties which must ultimately be explained'. There were enough front-rank physicists and chemists in the laboratory for a strenuous discussion. Langmuir stressed the chemical evidence. Stereochemistry required directed primary valence forces, in three dimensions. Upon four postulates concerning the spherical shell structure of the rare gases, accounting for the atomic numbers of these elements, he built three other postulates dealing with the forces and tendencies which govern the arrangement of electrons in the outside layer of atoms other than those of the rare gases. With these postulates he proceeded to 'derive the properties of the chemical elements'. In four further postulates he discussed stable pairs of electrons and a stable arrangement of eight, the octet. This led directly to the octet theory of valence. This theory 'explains the periodic properties of all the elements including those of the eighth group and the rare earths'. It was applicable to Werner co-ordination compounds. It defined the similarity of compounds like carbon dioxide and nitrous oxide, isosteres, containing the same number and arrangement of electrons. It accounted for such unusual cases of isomorphism as the crystallographic properties of sodium nitrate and calcium carbonate provide. He pointed out that ordinary valence theory predicts isomorphism (sulphites and carbonates) where none exists. Isomorphism between complex inorganic compounds,

such as between Na_2BeF_4 and $\text{MnCl}_2 \cdot 4\text{H}_2\text{O}$ provided additional confirmation of the applicability of the octet theory.

The discussion raged fast and furiously. The chemists wanted a 'static' bond to explain valence and molecular structure. The physicists could not tolerate 'static' electrons and retain their classical concepts of mechanics. Lewis and Langmuir versus the physicists gave rise to spectacular discussions. These took place just before the advent of wave mechanics and the quantum mechanics. Kossel's theory of the electrovalent bond producing inert gas configurations, Lewis's theory for the non-polar bond, the shared pair of electrons, and Langmuir's vigorous presentation of all these in his octet theory gave way to electrons in shells or energy levels at various distances from the nucleus, to electron clouds. The Pauli Exclusion Principle provided the key to the problem of the Periodic Table and the nature of the chemical bond. The Heitler-London theory gave the valence-bond approach to molecular structure while Hund, Mulliken and Lennard-Jones were developing the method of molecular orbitals. Chemistry and physics gained a unified approach and deeper comprehension of the structure of matter.

Langmuir was awarded the Nobel Prize in 1932 for 'his discoveries and researches in the realm of surface chemistry'. His colleagues in the General Electric Company have emphasized in their tribute to him his unsurpassed qualities as a physicist.* His doctoral thesis on heat-loss from electrically heated wires in gases was his earliest research effort, leading in Schenectady to its formulation in terms of conduction in a stationary film around the wire with convection outside of this film. The application of this to nitrogen-filled lamps and helical wires has already been discussed. His studies with tungsten filaments of the Richardson equation for electron emission as a function of temperature revealed that the electron emission, close to the melting point of tungsten, was many orders of magnitude less than that predicted by the Richardson equation. A saturation emission was reached at a lower temperature dependent on the voltage. The result of these observations was Langmuir's law of electron space charge which provided the basis upon which the electronic tube developments could be secured. Space-charge formulae for cylindrical and spherical electrodes and the voltage gradient in electron tubes were carefully calculated. He noted the important voltage minimum resulting from the initial velocities of the electrons.

Electron emission studies led to the finding of the enormous increase in electron emission from tungsten filaments which contained thoria to the extent of 0.5 per cent. The addition of thoria aimed at the prevention of crystal growth of the tungsten metal, the prevention of 'off-setting'. Langmuir traced the effect on the electron emission to the partial dissociation of the thorium oxide by the tungsten with release of metallic thorium which migrated to the filament surface. In the form of a layer adherent to the underlying tungsten, it had a lower work function than thorium metal.

* The following paragraphs owe much to the tribute, by his colleague Dr A. W. Hull, to Langmuir in *Nature, Lond.*, **181**, 148 (1958).

Thoriated tungsten filaments thus became the 'standard components of today's medium power tubes'.

Every experimentalist requiring high vacua has, since 1915, had occasion to appreciate the Langmuir condensation pump. It grew out of the work of Gaede who, in that year, published his account of the 'diffusion pump'. Langmuir realized that the action of the pump involved, in actuality, the mechanical transport of the gas being evacuated, propelled by mercury vapour atoms. Immediately after World War I Langmuir's designs for a 'condensation pump' became familiar tools in research laboratories, boiling mercury producing a stream of mercury vapour which swept the gas before it. The mercury was then condensed by water-cooling and returned to the boiler. From the research laboratory it passed to industry as an essential element in every high-vacuum operation, as a rapid, high-speed, vacuum pump of great capacity.

During World War I, Langmuir was concerned with the problems of submarine detection with a station on the New England coast, Schenectady acting as a development laboratory. It was during these years that he revealed to the man-in-the-street his uncanny ability to turn observations to useful account in the solution of war-time problems. The unhesitating progress of an unassisted blind man across a busy street in Boston, the behaviour of bats deep in mountain caves far away from any light sources became tools for Langmuir in the solution of anti-submarine problems.

The early 1920's were occupied by two experimental themes, low pressure gas discharges and his classical studies of the adsorption of caesium on tungsten. Of the first it must suffice to record that it led to a definition of the zones in the discharge, the correct measurement of potentials in the discharge using either a heated electrode or alternatively a cold electrode as probe. His quantitative theory of the positive ion sheath showed why a negative voltage on the grid of a thyratron cannot interrupt the discharge, since each grid wire is surrounded by a thin positive ion sheath that insulates it from the discharge.

With caesium on tungsten, he related the work function of a clean wire (4.6 volts) to the ionizing potential of caesium vapour (3.9 volts). At high temperatures, every caesium atom striking the filament left it as a positive ion. Below 1100 °K, however, no positive ions were formed. The influence of the adsorbed caesium was to lower the work function below 3.9 volts. From a thoriated filament with work function of 3.1 volts there was also no positive ion emission. Using the Saha equation for thermal ionization of a gas and a two-dimensional gas layer of mobile adsorbed atoms he was able to predict accurately the emission of electrons, ions and atoms from the adsorbed layer as a function of filament temperature. As a by-product of his study he showed that the rate of evaporation of the adsorbed species varied exponentially with the fraction of the surface covered: $v_{\text{des.}} = k e^{-h\theta}$. The bibliography appended to this article reveals that these researches on gas discharges and caesium films continued well into the 1930's.

To the same decade belongs the development of the hydrogen welding torch. This stemmed from the observations of R. W. Wood that, in a low-pressure hydrogen discharge, a platinum wire or even particles of dirt in a part of the tube remote from the discharge could become white hot. Langmuir correctly concluded that hydrogen atom recombination was responsible and that the phenomenon could be put to use with an arc discharge in hydrogen propelled against metal parts to be welded. A U.S. Patent, No. 1947 627 was issued in 1934.

In the early days of World War II three different problems were occupying his attention. His hobbies, a study, in summer, of the relationships between wind velocities, waves and currents on Lake George, N.Y., and, in winter, the formation and disappearance of ice in the lake, his studies of clouds, their formation, the extent of super-cooling and the associated meteorological problems all contributed to the solution of the problems now engaging his attention. His studies of icing and his measurement of particle size by means of Rayleigh's law of light scattering led to his production of 'white' smoke as a smoke-screen. Using Rayleigh's law to calculate the optimum size of particle, he developed, with V. J. Schaefer, methods of producing smoke particles of this size. In this way, it was possible to produce, from a gallon of oil, smoke screens several orders of magnitude greater than the coarse 'black' smokes previously used. The capacity of gas masks to filter out particles of a given size also engaged Langmuir's attention. His final research activity is indicated by the titles of his papers published in the post-war period. They include the production of snow from supercooled clouds, the production of rain by a chain reaction in cumulus clouds at temperatures above freezing, the control of precipitation from cumulus clouds by various seeding techniques including, *inter alia*, solid carbon dioxide, silver iodide and sodium chloride. He noted a seven-day periodicity in the weather of the U.S. during April 1950. These studies led, shortly before his death, to an exhaustive mathematical study of the probability of induced rainfall produced by periodic seeding. His close collaborator over the years, and more especially in the meteorological work, was Dr V. J. Schaeffer who, after a high school education, had begun his scientific career in 1926 at the age of 20 in association with Langmuir in his laboratory at the General Electric Company in Schenectady.

Irving Langmuir's grandfather emigrated from Glasgow to Toronto, Canada. There were several children of this grandparent all of whom remained in Canada with the exception of Irving's father, Charles, who came to the United States and there married Sadie Comings, the daughter of a doctor from Georgia. There were four children, all boys, of which Irving was the third son, his older brothers, Arthur C., an industrial chemist, Charles H. a vice-president of the New York Life Insurance Co., and a younger brother Dean, who became an investment broker. Arthur has told us that 'Irving owes his enthusiasm, freshness of outlook, and tremendous curiosity to his mother, and his surefootedness and common sense to his father'. When over 70 his mother, a widow, made a trip around the world. Two of his nephews,

David B. and Alexander D. Langmuir, credit their father, Charles Herbert, with recognizing the 'finely organized mentality' and scientific potentiality of his next younger brother. The eldest brother, Arthur, would have had him become, like himself, a successful industrial chemist. Certain it is that Irving's orientation towards science was mainly due to Arthur's encouragement and direction. In the issue, Irving compromised. He abandoned teaching but found, in an industrial environment, the perfect setting for basic research. He provided ultimately a convincing answer to Herbert's question posed in 1904, while Irving was still a student at Göttingen '... whether you have, or have not, exceptional ability in pure science research. If you simply have a well grounded knowledge and a thorough efficiency, you should certainly go right into the *business* of chemistry... But, if you are the exceptional man, it is, in my opinion, your duty to be one of the pioneer scholars in America... It is time for the young men of brains to be pioneers in the cause.'

Three years after Langmuir went to Schenectady he married Marion Mersereau, in April 1912. There began thus an ideal companionship, a community of interests, shared closely together—mountain climbing in the Rockies as well as in Switzerland, some sailing together in a small boat alone. His wife records that Irving was a skilful navigator. At one time they had their own plane. Langmuir loved to fly and was an excellent pilot. To this I can testify. My first flight with an amateur pilot was with Langmuir in his plane. He navigated superbly round a thunder area on Lake Erie, out of Cleveland en route to Schenectady. His skill as a pilot was as convincing as his presentation of his latest findings in science. He discontinued flying after a few years because he said it 'interfered with his work'. But, his wife remarks, 'we both had great fun while it lasted'.

The Langmuirs had two children, Kenneth who lives in Tucson, Arizona and Barbara, Mrs Harry R. Summerhayes, who lives with her husband and two sons in Schenectady. One of Langmuir's lovable qualities, as his wife testifies, was 'his devotion to children—he had a great love for them, endless patience in talking to them, never on a "higher plane"'. Part of his greatness was his simplicity and his enthusiasm in giving himself to others. 'He was a great lover of music... had a remarkable memory for every piece he heard, operatic or otherwise.' His family shared his love for music. Numerous colleagues, nephews, nieces and friends share his wife's memory of her gifted husband. Of him she wrote that 'he was as great a husband and father as he was a scientist'.

World wide recognition came to him for his work. In addition to the Nobel Prize in 1932 Langmuir was elected a Foreign Member of the Royal Society in 1935. Already in 1918 he was chosen Hughes Medallist of the Society and gave the Pilgrim Trust Lecture to the Society in 1938. The Chemical Society of London, academies in France, Italy, Sweden, Brazil, and India, among others, honoured him with election to membership and to coveted medals and prizes. Fourteen universities gave him honorary degrees including

Oxford, Edinburgh, Harvard, Princeton and the Johns Hopkins University. 'The measure of a scientist is the originality and fruitfulness of his ideas.' Langmuir's many honours and distinction are eloquent testimony to his stature as a scientist so defined. He has left a permanent imprint on the science he so generously served.

HUGH TAYLOR

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J B Leathers

JOHN BERESFORD LEATHES

1864-1956

JOHN BERESFORD LEATHES who died at Montreux on 14 September 1956, in his 92nd year, was born on 5 November 1864. He was the second son of the Reverend Stanley Leathes, D.D., a Professor of Hebrew at King's College, London, and Prebendary of St Paul's. His mother was Matilda Butt, who counted among her ancestors a Dr Butt, a physician to Henry VIII. His brother was Sir Stanley Leathes, K.C.B. He married Sonia Marie Natanson in 1896 in London, a Russian by birth, and a fine pianist. They had one daughter Margaret, who married Lionel Penrose, M.D., F.R.S., Professor of Genetics at University College, London. Leathes was educated at Winchester College, being a classical scholar from 1878 to 1883. While there, he had, as Prefect of Hall, to deliver the speech on the occasion of the visit of Mr Gladstone. This was in Latin and Mr Gladstone replied in English. Leathes was also Captain of the Winchester College football fifteen in 1883. His interest in classics was lifelong, and gave him pleasure in his retirement. It is said that on one occasion, he surprised Sheffield University Classical Association with a learned and delightful paper on Greek poetry. At Winchester, it is recorded that there were no science laboratories when he started his education, and evidently he had little teaching in science there. Following Winchester, he went to New College, Oxford, as a Scholar from 1884 to 1888, where he studied classical moderations and the school of Literae Humaniores, obtaining a 2nd Class, and was a member of the New College Barge in 1886. Up to this time, Leathes was destined for the Church, but he felt that he would be more use to society as a doctor than as a clergyman, and he therefore took up the study of medicine. This was most unwelcome to his father, who was not prepared after this to contribute more than a shilling a week which he spent on his lunches. He lived with an aunt at Highgate who gave him breakfast and supper, and he walked the 12 miles daily from there to Guy's Hospital and back for his clinical studies. This was indeed a measure of his determination to pursue medicine. One wonders how many today would show such determination. He qualified as B.M. in 1893 and became as F.R.C.S. in 1894, though it is doubtful whether he would have been able to practise surgery as his eyesight was not good. In fact, he formed a great friendship with E. H. Starling at Guy's, and under his influence, was persuaded to specialize in physiology, working with him in 1890-91 on the action of veratrine while acting as an Assistant Demonstrator of Anatomy; in 1894-95, he acted as a Demonstrator of Physiology under Starling at Guy's Hospital. Following this, as he was attracted to the chemical

side, he spent 2 years with Drechsel in Berne, 1895-97, and then two years with Schmiedeberg in 1898 and part of 1899 working on the mucoids of ovarian cysts. In 1899, he returned to England to hold the appointment of Lecturer in Physiology at St Thomas's Hospital Medical School. During eight of these ten years, he also held a part-time post at the Lister Institute, where he worked in enthusiastic companionship with C. J. Martin, Arthur Harden and Young, Cathcart, H. D. Dakin and H. S. Raper. These were happy days to which he often referred with satisfaction and it was at this time that his lifelong friendship with Raper began, and during which he delivered the course of eight lectures in the physiological laboratory at South Kensington, which became the basis of his well-known book *Problems in animal metabolism*. At this time he also came to know F. A. Bainbridge, P. Hartley and E. L. Kennaway.

In 1909, a new Chair of Pathological Chemistry was instituted in the Faculty of Medicine at Toronto, and Leathes was appointed Professor, being joined by H. S. Raper as a lecturer in 1910. His influence in the School was an important one. In addition to his regular work, he founded a local Medical Research Society which held regular meetings for the reporting and discussion of problems under investigation by the younger members of the staff in the Department of Pathology and Pathological Chemistry. In 1912, a new building adjacent to the Toronto General Hospital was opened, housing the Department of Pathology and Bacteriology and the Department of Pathological Chemistry. In this latter, Leathes established laboratories for graduate students interested in a course of teaching and research in chemical medicine. In collaboration with Dr Alexander McPhedran, then Professor of Medicine, a biochemical laboratory was established in the Medical Wards of the Toronto General Hospital and also of St Michael's Hospital with a member of the staff of Leathes' department in charge of each laboratory. This shows how alive Leathes was even at this early time to the value of the clinical biochemist. In 1911, Leathes was elected to the Fellowship of the Royal Society. He was invited to come to Sheffield by the Vice-Chancellor, H. A. L. Fisher, and accepted the post of Professor of Physiology in succession to J. S. Macdonald, leaving Toronto in December 1914, that is to say after the start of the First World War. This Professorship he was to hold until his retirement in 1933, and he acted as Dean of the Medical Faculty in 1916 to 1922 and from 1931 to 1933. During the World War I, he was in charge of nephritis wards of the 3rd Northern General Hospital, and was actually appointed to the staff of the Hospital as Honorary Physiologist, as well as being responsible for the establishment of biochemical laboratories in 1919 in the two general hospitals.

After leaving Sheffield in 1933, Leathes moved to Wantage and then to Oxford for a short time to work with Professor John Mellanby, F.R.S., in the laboratory of physiology. The Second World War disturbed this arrangement, so that he moved to Lyme Regis, subsequently to London and Southbourne, and finally settled in Switzerland where he had many associations; it was

there that he had first met Mrs Leathes. The Universities of Manchester and of Sheffield conferred the Honorary Degree of D.Sc., and he was also elected an Honorary Fellow of his old college at Oxford. In 1921, he became a Fellow of the Royal College of Physicians.

Both during the time in Toronto and later in Sheffield, Leathes showed himself much alive to the importance of breaking down the barrier between preclinical and clinical studies so that the modes of thought in the laboratory could be brought into clinical medicine. It is said that he showed much tact, infinite patience and a transparent honesty of purpose which effectively overcame opposition. In Sheffield, the medical curriculum was revised so that the students continued physiology studies into the final year and the applications of physiology were taught at the bedside. In this way, where possible, disease became explained as a diversion from the normal. It was indeed fortunate that part of his time in Sheffield included the appointment of Edward Mellanby as Professor of Pharmacology from 1920, and it has been said that Leathes and Mellanby produced some of the first insulin to be made in this country for clinical use.

He represented Sheffield University on the General Medical Council from 1919 until 1938, and was for 10 years an editor of the *Journal of Physiology*. As colleagues or pupils, he was associated with the following Fellows of the Royal Society: H. S. Raper, P. Hartley, E. L. Kennaway, E. P. Cathcart, J. B. Collip, Sir Frederick Banting, F. S. Bainbridge, E. J. Starling and Sir Edward Mellanby.

More than that of most scientists, the published work of Leathes falls into the three categories of research, lectures and books. Much of his original thought is to be found in the latter two categories. It will be noticed that his papers published alone or with colleagues start with papers with Starling on pleural fluid and on absorption from the intestine. In his early period, he also devoted attention to purine metabolism, making some interesting observations; one which the writer does not think has been repeated is the effect of shivering upon output of uric acid in relation to creatinine. This was done with Cathcart, and the upshot was that an abnormal increase of uric acid was observed. After these rather widely scattered researches towards the chemical side of physiology, he settled down to work upon the fats. Quite early the series of lectures given in 1904 and published under the title of 'Problems in animal metabolism', stamped Leathes as a writer and thinker of importance. In fact, this book was recommended to all students in 1908 to 1914. Throughout his life, Leathes's personality impressed itself on the students with whom he came into contact, and the evidence is very strong that he was to be ranked as a great teacher. Even in the case of one who did not get on with him well when young, and this was very unusual, this same person in his old age realized the impact of Leathes upon his life. From the period 1906 to 1928, Leathes seems to have concentrated mainly upon the fats, in which field he published researches as well as the classical monograph with Raper on *The Fats*, which appeared in 1910, and ran to a second

edition in 1925. On the same subject, he published the Harvey lecture in 1910 on the functions of the liver in metabolism, and then the Croonian Lectures given to the Royal College of Physicians in 1923, again on 'The role of fats in vital phenomena'. The communications as distinct from the rest include a paper showing that there may be a definite small increase in estimated fat in the liver when this is kept under aseptic conditions after death, a comparison of the amount of fat in different muscles, and finally the paper with Meyer-Wedell, which formed the basis of his view that the liver desaturated the fatty acids. These observations that the liver fats were more unsaturated than those in the rest of the body were later confirmed with more modern technique by Schonheimer. There is no doubt that this view stimulated much research over the years, and that it still attracts attention as will be seen by the remarks in the recent monumental work of Deuil on the fats (volume 3). At the same time, there are other interpretations, which have become necessary as the result of the present knowledge that the highly unsaturated fatty acids such as arachidonic acid, which are also known as the essential fatty acids, cannot be synthesized in the body. This means that in any case, the liver would not do more than put in one double bond. Here again, it is more likely, as the result of present biochemical research, that this double bond would be in the $\alpha\beta$ position and not in the position which it occupies in oleic acid. So that the question is raised whether the liver concentrates in the main unsaturated fatty acids brought to it from elsewhere. This would diminish the significance of the idea that the first step in the degradation of the saturated fatty acids was dehydrogenation, but at the same time, it does not detract from the value of the theory in stimulating research.

After 1918, Leathes became attracted by the recent work upon fatty acids in surface films, arising in the first instance from the observations of W. B. Hardy, followed by those of Langmuir and Harkins. He was therefore quick to appreciate the importance of the work being done at the time by N. K. Adam at Sheffield on surface films in his capacity as Sorby Fellow, and devoted attention to the behaviour of lecithin (always one of his interests) and of cholesterol on the Langmuir trough. He showed that these films were expanded at ordinary temperatures, but became more closely packed if cholesterol was present. These observations are mainly embodied in his Croonian Lectures, where he puts the subject into focus in a penetrating discussion of problems of permeability in cells in relation to what would be called now the phospholipids. A lecture of particular interest is that in which he shows the behaviour of lecithin films when exposed to water containing various substance. Myelin forms grow out which may eventually engulf aqueous phases by becoming hydrophilic. The writer thinks that this work might be studied again with profit, now that so much more is known about the problems involved in the metabolism of this class of fatty substance. In any case, they should be borne in mind by those working upon the movements seen in unicellular organisms and in tissue culture. It was a thesis of Leathes that the fats were of predominant importance in cell life, and that

they could not be considered merely inert. The book on the fats (monograph) with Raper became a standard work of reference at that time for those working in the field of fats.

Another aspect of Leathes is brought out by his address to the Physiology Section of the British Association at Oxford in 1926, which the writer had the privilege of hearing. This was an outstanding philosophical discussion on form and design in physiology. With it may be coupled some of the thoughts to be found in an address to the Sheffield Medical Society given in 1933, and published later. The theme is that there is form and chemical arrangements but that we must never be too complacent owing to the enormous complications of physiology. He gives as instances that the number of possible arrangements of the amino acids to form the proteins are 10^{48} . In their approach to the complications of biology, there seem to be two main types of mind, if one excludes those which are purely superficial. One type finds the complications so great that they can never set aside these in their thoughts. The other realizes, at any rate in part, the complications, but they do not loom so large that they find these thoughts inhibitory in an attempt to get some simple chemical facts out of the cell. Both minds are clearly necessary in physiology taken in the broadest sense, and the role of Leathes was that of acting as a brake to too excessive enthusiasms. In this, he was following the integrity of his mind as influenced possibly by his early training in Oxford in philosophy. His last reports were published with J. Mellanby after he retired, when he came to work in the Oxford laboratory of physiology; they were interesting in showing the potentiating action of lecithin on the thromkinase action of daboia venom. Finally, the account would not be complete without reference to the Harveian Oration on 'The birth of chemical biology', which may be read with profit by all those who want to know about the early work of Boyle and Mayow.

Other work of his which should be mentioned is a paper in 1899 in which he found that the mucoid of ovarian cysts was related to chondroitin sulphuric acid. With E. P. Cathcart, he also studied the absorption of proteins from the small intestine (1906) taking up the conception that albumose and peptones were converted by the epithelial cells into blood proteins. They could not find albumose or peptones in the blood perfused through pieces of gut. When working with loops of intestine *in situ*, in an hour or two they found that an amount might be absorbed containing two or three grams of nitrogen, largely increased by the increase of non-protein in the blood. This led to the conclusion that proteins were converted to amino acid products of complete hydrolysis for absorption. In their work on uric acid, it was found that the excretion was greatest in the forenoon and lowest at night, whereas the total nitrogen output was greatest in the first part of the night. An attack of fever induced by typhoid toxin resulted in an output of uric acid at night almost four times as great as the normal. There were other works on the reaction of the urine and the excretion of water in trench nephritis.

Any account of Professor Leathes would be incomplete without reference

to work by himself and his close associates, much of which he inspired and directed. A reference to these papers will show that the majority dealt with the nature of the fat in liver, kidney and heart, in disease as well as in the normal. An attempt was definitely made to establish how far the fats present were unsaturated. Much of this effort was aimed at the attempt to test further the theory that the liver desaturated fats in preparation for their metabolism. One paper, however, deals with the behaviour of sphingomyelin, phrenosin and kersin in surface films.

This review of the life of a great and beloved physiologist would not be complete without mentioning the love of the country and the vigour with which he enjoyed his extensive tramps over the Derbyshire moors; and he was able to climb mountains in Switzerland until late in life. He was a keen gardener, sang in the Bach choir and took much interest in music and enjoyed his wife's piano playing. Furthermore, not many of us realize that he was considerably skilled in the use of pastel in his drawings.

The writer is indebted to many for information and advice, especially to Mrs Leathes, Mrs Penrose (daughter), Professor G. A. Clark, who succeeded Professor Leathes at Sheffield, Dr Fletcher of Toronto; to the Librarian of the Royal Society of Medicine for references and to Miss Frostick for secretarial help.

RUDOLPH PETERS

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W. C. McCallagh Lewis

WILLIAM CUDMORE McCULLAGH LEWIS

1885-1956

WILLIAM CUDMORE McCULLAGH LEWIS, who died at his home in Malvern on 11 February 1956—eight years after his retirement from the Grant-Brunner Chair of Physical and Inorganic Chemistry in the University of Liverpool—was a pioneer in research and made lasting and outstanding contributions to physical chemistry. He will always be remembered for his studies in the theory of chemical change and colloid science.

Lewis was born at Belfast on 29 June 1885 and was the only son of a family of five children. His father, Edward Lewis, was a native of Belfast and a linen merchant in that city; his mother, Francis Welsh McCullagh, was the daughter of the Rev. William Cudmore McCullagh of Ballysillan Presbyterian Church, Belfast. Lewis married Jeannie Waterson Darrock, of a Scottish family who had settled in London and their only child, Ian, became a lecturer in physics in Liverpool University before joining the Atomic Energy Research Establishment at Harwell.

Lewis was educated at Bangor Grammar School, County Down and at the Royal University of Ireland (now Queen's University). He entered the University as a medical student but having early developed an interest in the physical sciences he changed his course and proceeded to obtain a First Class Honours degree in Physics and Chemistry. In 1906 he was awarded the M.A. degree and a University Studentship in Experimental Science. After acting as Demonstrator in Chemistry at the Royal University for a year he left Northern Ireland to continue research at the University of Liverpool under Professor F. G. Donnan—himself an Ulsterman and a first cousin of Lewis's father. Donnan, who two years earlier had been appointed to the new chair of Physical Chemistry in Liverpool (the first in the country to be devoted solely to that growing subject) was fresh from working with the great physical chemists van't Hoff and Ostwald, and with all the new research techniques of physical chemistry at his command soon gathered from his own students a group of researchers which included F. A. Freeth, A. J. Allmand and many others, all of whom subsequently became leaders of physical chemistry in England. It was in this environment that Lewis began his first real physical chemical research and there is no doubt that his career and his interest in colloid science was profoundly influenced by the inspiration, friendliness and ability of Professor Donnan and his co-workers. After completing, with Donnan, a highly successful experimental examination of William Gibbs's theory of surface concentration, Lewis was awarded a scholarship which took him to Heidelberg to work for a year with the

distinguished colloid chemist Bredig. In 1909 he returned to England and was appointed by Sir William Ramsay to a Demonstratorship and later a Lectureship at University College, London. Lewis's contributions to physical chemistry had by now attracted considerable attention and it was not surprising that in 1913 he was invited to succeed Professor Donnan—who had taken Ramsay's Chair at University College—in the Chair of Physical Chemistry in Liverpool, a position he held until his retirement owing to ill health in 1948.

During his long association with Liverpool University, which extended over nearly forty years, Lewis gave devoted service to the University and directed one of the outstanding schools of chemistry in this country. He was a friendly professor, always ready to give advice, hear sympathetically the difficulties of his students and colleagues and help them on the way to a successful solution of their problems. He inspired young students and the large number of his former research assistants who have attained eminence in academic and industrial spheres bears testimony to his teaching and influence. He was a man of wide learning and considerable breadth of outlook; an ardent student of Dr Samuel Johnson and also interested and knowledgeable about early English architecture. Being brought up in the Presbyterian tradition, he remained throughout his life an active and regular churchgoer.

Although he always felt that his work was hampered by insufficient mathematical training, much of his best work was of a theoretical nature and his early studies on the nature of chemical mechanism and catalysis pointed the way to the use of the methods of statistical mechanics in chemistry. In 1918 he proposed a theory of chemical change which was to form the basis of subsequent developments in this subject. To physical chemists Lewis was widely known as the author of *A system of physical chemistry*. This textbook, first published in two volumes in 1916, and as three volumes in 1919, was the first original work in the English language to be devoted to physical chemistry for senior students. It was the standard work for students in this country for two decades and went into four editions in ten years. The 'system' of classification upon which the book was based was the natural subdivision of physico-phenomena into two classes: first, the phenomena exhibited by material systems when in a state of equilibrium (vol. II); and secondly, the phenomena exhibited by material systems which have not reached a state of equilibrium (vol. I). In this way Lewis deliberately set out to avoid the common 'selected chapter' method of treatment, which may be necessary for the preliminary accumulation of facts but is liable to cause the student to lose sight of the essential unity of the subject. The third volume added to the second edition (1919) was an account of the quantum theory in its physico-chemical aspect and introduced the student to the important role which quantum theory was then playing in physical and chemical research. This book, written at a time when the subject of physical chemistry was rapidly growing, exerted a wide and lasting influence on the subject in this country.

The task of preparing it was a prodigious one and revealed a breadth of outlook possible only in a man who had the widest grasp of the many developments occurring at that period.

Soon after the first world war there was formed in Liverpool the 'Liverpool Cancer Research Organization' which later became the 'Liverpool Medical Research Organization' under the directorship of Professor W. Blair Bell. The plan of operation was that the heads of the various departments in the University whose expert knowledge could be brought to bear on different aspects of the same line of attack were consulted, and invited to take part in the work of the Organization in their own laboratories with the aid of research assistants and facilities. Lewis responded eagerly to this idea and devoted himself to the common task. With the help of a team of whole-time associates he undertook the necessary research on the preparation of materials for use in the treatment of patients, the fate of lead in the blood stream and the arduous and difficult work of trying to establish the similarity of chemical composition and the function of the chorionic epithelium and malignant tissues. Lewis was interested in the physico-chemical processes which underlie malignancy and studied such properties as the electrical conductivity and ionic permeability of malignant tissue, pH variations of the blood, enzymatic activity, and glucolysis in malignant tumours. These investigations influenced Lewis's future work and from that time he transferred his research interests to the study of biological and physiological problems.

Lewis was extremely retiring, hated publicity and in his later years rarely attended scientific meetings outside of his University. Dignified in appearance and correct in manner, Lewis will be remembered for his sincerity and kindness. He had a keen sense of humour and he was wont to enliven many a grave discussion with flashes of Irish wit. He was devoted to University ideals and to research in particular; it was his constant concern that every opportunity should be given to his staff for such work and that they should be given full credit and recognition for their achievements.

RESEARCHES IN CHEMICAL KINETICS

During his first decade in Liverpool Lewis led an active school in experimental studies of chemical kinetics. The most notable single publication of a long series of papers on the mechanism of chemical change was his 'Calculations in absolute measurements of velocity constants in gaseous systems' (1918). Although not all the conclusions of this paper have been proved to be correct this work gave a considerable impetus to the understanding of chemical change. Lewis was one of the first physical chemists to appreciate that velocity constants in chemical systems could be treated by the methods of statistical mechanics and quantum theory. He was interested in slow reactions occurring at finite and reproducible rates in which only a small fraction of the molecules became reactive in unit time. It was very evident

that these molecules existed in some state very different energetically from the average state. In an address to the British Association in 1915 Lewis enunciated the fundamental problem of the specific nature of reaction velocity and the remarkable effects brought about by a change of temperature. Accepting the Arrhenius concept of 'active molecules' and that the law of variation of rate with temperature could be explained if a fraction of the molecules existed in a higher energy state, Lewis sought an answer to the nature of this energy in terms of the quantum theory. He believed that the process of activation was by direct interaction with the radiation field. This view seemed very reasonable when one considered the large number of photochemical reactions in which it was necessary for one of the reactants to become activated by absorbing light of a characteristic frequency. Lewis made the significant and fundamental assumption that reaction was promoted by the absorption of radiant energy and that the energy of activation was given by $E = Nh\nu$ where ν is the frequency of the absorbed radiation, h = Planck's constant and N = Avogadro's number. The velocity of reaction was assumed to be proportional to the density of radiation and this hypothesis was in broad agreement with the experimentally observed rate law. This theory had a strong following for many years and although one must admire the boldness and beauty of this conception which was an attempt to unify our picture both of photochemical and thermal reactions, experimental evidence rapidly accumulated which showed that the radiation theory was untenable. It received little following after about 1926, but this is not the proper place to discuss the evidence which finally resulted in its downfall. In the important paper referred to above (1918), Lewis formulated the main principles of the collision theory in the form which has since been generally accepted except for the interpretation of the real meaning of the activation energy E . He showed that in certain examples the rate of chemical change was simply the rate of molecular collisions multiplied by the fraction of the total number of molecules having sufficient energy. He tested this theory by evaluating E from the Arrhenius relationship using this value to calculate the velocity constant k and comparing it with the experimental value. The theoretical studies of Lewis were backed by extensive experimental measurements on reaction kinetics, particularly hydrolytic reactions. In this work he recognized the limitation of the classical theory, and the importance of activity and salt effects.

RESEARCH IN COLLOIDAL AND BIOLOGICAL CHEMISTRY

From the time of his association with F. G. Donnan, Lewis was keenly interested in colloidal problems and the possibility of interpreting colloidal phenomena in physico-chemical terms. The wide variety of investigations in this field which he carried out with a succession of research students may be considered broadly under the following main headings The adsorption

of solutes at liquid surfaces and interfaces; the physical chemistry of biological substances and systems; the coagulation of gold sols; the charge and stability of oil droplets in aqueous media; and the electrophoretic behaviour of dispersed organic compounds. Much of this work was brilliantly conceived and, although not always so successful as Lewis would have wished, it may be said that some of the projects undertaken were years in advance of their time. Techniques however had not been then developed for their successful prosecution and greater success would certainly have been attained if the work had been carried out at a later date.

About 1908, as one of the group of workers drawn together by F. G. Donnan at Liverpool, Lewis made one of the earliest attempts to confirm the Gibbs adsorption theorem by the then novel method of determining the adsorption of solutes at a hydrocarbon oil/water interface. The experimental adsorption values were many times greater than the Gibbs values for the solutes sodium oleate, sodium glycocholate, Rosaniline and Congo violet, and Lewis suggested irreversible aggregation on the oil surface as a possible explanation of the discrepancy. For caffeine the experimental adsorption agreed fairly well with that predicted by the Gibbs equation. A considerable number of salts were also found to reduce the tension at the hydrocarbon/water interface, and for KCl, AgNO₃, BaCl₂ and CaCl₂ the experimental adsorption was of the same order as the Gibbs values.

Inorganic salts were known to raise surface tension yet, rather surprisingly, Lewis had observed them to lower interfacial tension; no inversion of the surface tension effect was obtained between 10 and 80 °C. He explained the apparently different effects of inorganic salts at the air/water and oil/water interfaces in terms of Laplace's capillary factor and theory of internal pressure.

Through this work Lewis was led to consider the nature of the transition layer between two phases. The internal pressure values required in the application of Laplace's theory differed considerably according to whether they were calculated from van der Waals's equation or from the work required to remove unit volume from the surface layer of liquid outside the range of molecular attraction. The divergence could be resolved by attributing an enhanced density to the surface layer of a liquid. On this basis he calculated the average surface density of water, and some 15 other liquids, to be about two and a half times the normal bulk value.

Following this early work, many years elapsed before Lewis returned to study afresh the effect of electrolytes and other solutes on surface and interfacial tension. In the years between, Harkins and co-workers developed the drop volume technique into one capable of high accuracy. It was with this technique that most of the later investigations were carried out by various workers under Lewis.

Reinvestigating the effect of electrolytes on oil/water interfacial tensions, A. W. Evans (1937) found that whilst NaOH and LiCl raised the tension at the *n*-hexane/water interface, KCNS lowered it. W. L. Guest (1939) found that KCl, LiCl, BaCl₂ and LaCl₃ raised the trans-decalin/water tension,

whilst KI and KCNS lowered it; the effect of iodide and thiocyanate ions was attributed to specific adsorption of these ions. W. Dickinson (1940) combined interfacial tension with electrophoretic measurements for cetyl acetate/water and confirmed the above effects for KCl, KI and KCNS. Quantitative analysis of these results gave the surface concentrations of the electrolyte ions separately, and Cl^- , C^- and CNS^- were shown to be positively adsorbed, whilst there was a desorption of K^+ (Dickinson 1944).

In the years following the first world war Lewis collaborated with Blair-Bell in a programme of cancer research, and thereafter remained keenly interested in the physical chemistry of biological materials and systems.

Blair-Bell believed that in cancer the malignant cells reverted to the type of chorionic epithelium and sought to control and kill the cancerous growth by the use of colloidal lead, which provided a continuing supply of lead ions at very low concentration. In this way it was hoped to minimize the toxic effect of lead on normal tissues and function, whilst permitting it to act on the malignant cells. Lewis and co-workers prepared the lead sols used clinically and undertook an extensive physico-chemical and biochemical examination of the composition and function of chorionic epithelium, and of malignant and normal tissues.

Malignant tissues were found to have a higher electrical conductivity, a higher K^+ and lower Ca^{2+} content than normal tissues. The increased permeability of the malignant tissues was attributed to the high lecithin/cholesterol ratio they were found to possess in comparison with normal cells. The ratio for cancerous tissue was in the same direction as that for chorionic epithelium, thus lending support to Blair-Bell's views. In view of the enhanced permeability of cancer tissue to water soluble substances, Lewis considered that the pH of the blood and tissue of normal and diseased persons might show significant differences, but none was found. The intravenous injection of lead sol produced a temporary lowering of blood pH, which then returned to its normal value.

From a survey of the literature it appeared to Lewis that of the chemical processes of oxidation, reduction, hydrolysis, proteolysis and glucolysis as they occurred in cells, only the last was markedly different from normal in cancerous tissue. Cancerous cells showed appreciable glucolysis under aerobic conditions, as did chorionic villi, whereas normal cells did not. The mechanism of glucolysis was thought to involve the enzymic hydrolysis of cell glucosides to produce an 'active glucose' as intermediate, which was converted by subsequent processes into lactic acid. Lewis therefore instigated an investigation into the acid and enzymic hydrolysis of a number of glucosides, including phloridzin. This work was largely carried out by Moelwyn-Hughes, who also studied the reactivity of glucose in acid solution to determine, if possible, the nature of 'active glucose'.

Other enzyme experiments carried out in connexion with the cancer programme related to the effect of sera from normal and diseased persons on the activity of pancreatic lipase. Both normal and cancerous sera were found

to increase the hydrolysis rate, the former more than the latter, and the presence of ionic and colloidal lead enhanced these effects. This phase of the work was rounded off by a brief investigation of the kinetics of enzyme reactions, in which the degradation of casein by trypsin was followed experimentally.

It was perhaps inevitable that the above activities should have led Lewis to consider the properties of proteins from a physicochemical point of view. Thus during the 1920's a number of workers under him carried out investigations relating, in particular, to the coagulation of proteins by heat.

His approach was through kinetic studies and the acid-base titration characteristics of the fresh and the denatured protein. He concluded that, though denaturation was of the nature of a hydrolysis, the peptide link was not involved and he suggested that some spacial separation of NH_2 and COOH groups in one and the same protein unit was involved, which rendered them more available for union with groups in other protein units. His views remained essentially unchanged when he reviewed the problem in 1931. A final study of the effect of denaturation on the viscosity of egg albumin and oxyhaemoglobin solutions, in which the Einstein and Kunitz equations were applied, suggested that denaturation involved a swelling of the protein unit and increased hydration.

Through this work on the electrophoretic behaviour of the relatively complex substances lecithin, cholesterol and others, Lewis became keenly interested in the general possibility of relating electrophoretic behaviour to chemical structure. From the early 1930's to his retirement a number of workers systematically studied the behaviour of aqueous dispersions of a series of hydrocarbons, halides, alcohols, acids and esters, using a simple U-tube apparatus and adding sucrose to stabilize the boundaries. In general, the electrophoretic behaviour was found to vary characteristically from one homologous series to another but no overall interpretation was advanced.

The electrophoretic measurements on aqueous hydrocarbon dispersions were in part the outcome of Lewis's interest in the electric charge borne by hydrocarbon droplets as a possible source of the stability of pure emulsions. In his very earliest work he had noted that, irrespective of their method of preparation, the droplets in hydrocarbon dispersions appeared to lie within a fairly narrow size range with the majority between 0.5 and 1μ in diameter. During the 1930's he published papers in which this observation led him to seek an explanation of the size attained by the droplets, and in turn of their adherence and coalescence. He also attempted to calculate the maximum value possible for the electrical charge density at a number of organic liquid/water interfaces and at the mercury/water interface.

Passing from adherence to coalescence, Lewis realized that the decrease in interfacial energy which would result from the coalescence of two droplets to give one larger one was relatively so large that the process should always occur, unless opposed by some other factor. Lewis thought this was the electrical potential energy charge involved, but calculated that this also

should decrease on coalescence. Lewis concluded that above a certain critical charge or p.d., the charge would assist stability by mutual repulsion of the particles at distances beyond the range of the forces of attraction. With a sufficient decrease in double layer thickness resulting from the addition of electrolyte, however, collision might result in the approach of droplet surfaces within the range of the van der Waals or interfacial tension forces and coalescence proceed, now assisted by the resulting decrease in electrical potential energy.

Finally mention must be made of a valuable series of experimental studies on the coagulation of nuclear gold sols, induced by added electrolyte, carried out during the 1920's. These provided considerable evidence in support of Smoluchowski's theory of slow coagulation, but also indicated points of weakness in the theory especially as extended to deal with more rapid coagulation.

In the preparation of this memoir the writer has been helped greatly by Dr H. W. Douglas, one of Professor Lewis's former students. To him and to others who have given information the writer expresses his sincere gratitude.

C. E. H. BAWN

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Herbert Parsons.

JOHN HERBERT PARSONS

1868-1957

JOHN HERBERT PARSONS died on 7 October 1957, at University College Hospital, London, in his 90th year. He was primarily an ophthalmologist and over a long period of years held a unique position in that specialty. In the latter half of the nineteenth century and the beginning of the twentieth, British ophthalmology maintained the proud position among the countries of the world that it had inherited from Bowman, and its senior representatives were deservedly respected both at home and abroad—Jonathan Hutchinson, Nettleship, Doyne, Marcus Gunn, Treacher Collins, Fisher, Lawford, Paton and others. Among these, Parsons was a junior; but alone among them he grasped the significance of the changes that were at that time becoming apparent in the progress of medicine. They confined their attention to the study of ocular disease as it presented itself in the clinic, or followed up the ruins it left behind in the pathological laboratory; for him the real problem lay in neither of these but rather in an understanding of physiology and biochemistry, his interest was centred not in arranging disease-pictures in neatly docketed pigeon-holes, but in trying to understand the initial aberration from the normal. To a generation which hardly understood, he preached that advances in the treatment of cataract consisted not in the elaboration of operative techniques, but in an understanding of the biochemistry of the lens and its respiratory mechanism; of glaucoma, in the elucidation of the dynamics of the aqueous humour. Establishing himself in the front rank as a clinician in the years before the First World War, he spent the years between the wars striving to maintain the spirit of research in British ophthalmology at a time when it was conspicuously weak, widening the field of visual hygiene by adapting its lessons to industry, and extending his interests to the perceptual and psychological aspects of vision and sensory perception in their widest sense. During this period he dominated British ophthalmology by his great industry, his forceful personality, and his transparent integrity, and at the same time established a considerable reputation in wider scientific circles. And after the Second World War, when he formally opened the Institute of Ophthalmology in London and saw clinical and laboratory research married and set upon a secure basis, he had reason to believe that he had achieved one of the main purposes of his life.

Parsons was born in Bristol on 3 September 1868, the son of Isaac Jabez Parsons and Mary Goodhind Webb. He was the youngest of a family of five—three brothers and two sisters. Educated initially at Thomas Turner's private school, he attended the Bristol Grammar School (1884-6) and therein

obtained a Gilchrist Scholarship to enter University College, Bristol, in 1886. Here he acquired a wide educational groundwork in arts (Latin, Greek, French), science (mathematics, chemistry, physiology) and medicine, aided by a Stewart Scholarship and the first entrance scholarship to Bristol Royal Infirmary; thereafter he came to London where, at University College, he took an advanced course in physiology (1889). He graduated B.Sc. (London) in 1890 with honours in physiology and, completing his medical course at St Bartholomew's Hospital, he graduated M.B. in 1892. For a period, as Sharpey Scholar, he acted as assistant and demonstrator in physiology to Schäfer at University College. During his formative years as a student there is little doubt that his views were largely moulded in psychology by Lloyd Morgan, in physiology by Schäfer and Starling, influences which strongly flavoured his writings in after years.

After graduation in medicine, he started general practice in the London suburb of Finchley, and in 1894 married Jane Roberta Hendrice, the daughter of John Hendrice of Uddingstone, near Glasgow. But through physiology his interests centred progressively on ophthalmology, and he became a clinical assistant at Moorfields Eye Hospital. In 1900 he took the F.R.C.S. (England) and, aided by a British Medical Association Research Scholarship, he gave up general practice and threw himself into the whole-time pursuit of ophthalmology in its widest aspects with a determination of purpose and brilliance in achievement rarely equalled. Successively clinical assistant, curator and librarian at Moorfields, he was elected to the surgical staff of that hospital in 1904; in the same year he became assistant ophthalmic surgeon to University College Hospital, and for a time was ophthalmic surgeon to the Hospital for Sick Children, Great Ormond Street. To the first two of these hospitals he devoted his energies all his working life, maintaining a close interest in their affairs after he had retired from their staffs on account of age and had joined their consulting staffs; at the first of these hospitals, where a ward was named after him, on attaining the age of 60 he was pressed to remain on the active staff until the age of 65—an exceptional compliment at that time.

But it was not in hospital activities nor in practice that Parsons's heart lay; to him practice was not a first love and he was a good but not a brilliant surgeon. To him clinical ophthalmology was insufficient; he must correlate it with all the workings of the visual apparatus—its optics, its physiology, its neurology and its psychology; he saw the working of the eye in terms of physics and chemistry, of vision in terms of biology. To him it was impossible to conduct the routine of an ophthalmic clinic, to examine or refract a patient without a thorough understanding of the optics of the methods he was using—of retinoscopy, refractometry or ophthalmoscopy. The result was the appearance of his first book, a small manual, *Elementary ophthalmic optics* (1901). His interest in physiology was also maintained. His first important experimental work was a series of researches on the innervation of the pupil and the lacrimal gland. Stimulated by the work of Leber at Heidelberg and Starling

and Henderson at University College, he then undertook a series of researches in the department of physiology of that college on the vascular control of the intra-ocular pressure, work which was summarized in a second small book, *The ocular circulation* (1903). For these physiological researches he obtained the degree of D.Sc. (1904).

During this early period, however, his main interest was progressively directed to ocular pathology. As curator at Moorfields Eye Hospital he had grounded himself well in this subject, and, while contributing a series of original papers in the current literature on this branch of ophthalmology, he wrote his first classical treatise, *The pathology of the eye*, which appeared in four volumes (1904-8). In this immense work, full of original and critical observations, the whole of the literature on ocular pathology was correlated and for the first time was integrated into a self-contained discipline. The book, for its time, was superb, and through it Parsons established himself in a unique position and became recognized as a world authority. At the same time he published two translations from German works—Boldt's *Trachoma* (1904) and v. Hanke's *Treatment of diseases of the eye* (1905); while his maturing clinical experience was reflected in the appearance of his *Diseases of the eye*, an excellent, comprehensive and uniquely compact clinical manual for students and practitioners, which first appeared in 1907 and continually demanded new editions. This was undoubtedly his most popular work and it maintained an immense circulation both in this country and abroad; the tenth edition appeared in 1942, was reprinted in 1944, whereafter he transferred the authorship to other hands.

The considerable output of original and literary work of these eight years augmented by the publication of a large number of incidental clinical papers, was, of course, supplemented by heavy hospital responsibilities and the cares of a rapidly increasing consulting practice; but Parsons was a man of great physical stamina and untiring industry. At that period he worked to the exclusion of all other interests. He was now attracting considerable international attention, particularly as a result of his pathological work, and his clinics at hospital were much frequented by medical men from abroad. Here he tended to talk above the level of the average undergraduate, to forsake purely clinical matters and to dilate at length on the higher mathematics of optics, on abstruse problems concerning the physiology of vision or on pathological rarities. His clinic at Moorfields Hospital thus became a legend in America, but was relatively useless to the ordinary student seeking to acquire a working knowledge of his subject.

After the publication of his *Pathology*, Parsons forsook the laboratory and his original contributions became mainly integrative and philosophical in type, dealing essentially with the physiology and psychology of vision. This change of interest, for which his early training made him admirably fitted, arose in a practical way. As an ocular pathologist he had been asked by such bodies as the Royal Society and government departments to give advice on the deleterious effects of radiant energy on the eye and the value of adequate

illumination in industry. These activities, which form a story by themselves outside his main interests, will be noted subsequently; but arising therefrom the functional aspects of vision increasingly absorbed his attention.

In his capacity as consultant to the Board of Trade, the problems of colour vision and colour blindness assumed importance: incidentally, colour-blindness had been the subject of his first scientific paper in 1900. At the time this difficult and complex subject was in a chaotic state, its considerable literature a mass of papers written in support of numerous nebulous theories the formulation of which seemed to be the main interest of investigators. An unbiased assessment of the subject required someone with unusual critical ability and scientific honesty as well as a wide knowledge of physics, physiology and psychology. These Parsons possessed; and his second classical work, *An introduction to the theory of colour vision* (1915) was a masterpiece in the factual presentation of the subject. Today it is still indispensable as a work of reference. The biological and materialistic bent of his mind as well as his close association in work for the Board of Trade with Abney and Watson who continued the tradition of Thomas Young and Clerk-Maxwell, made him a strong advocate of the trichromatic theory of colour vision, a view-point he held to the end. This opinion he maintained resolutely, and to those who were interested in the subject and the controversies to which it gave rise, his life-long argument with Edridge-Green remains a distinct memory.

From colour vision his interests broadened to embrace the whole field of the psychology of perception. The first indication of this phase was the appearance of a small book, *Mind and the nation; a study in applied psychology* (1918). This was followed by the Bowman Lecture (1925) on 'The foundations of vision' and his philosophy was fully expounded in the greatest book he ever wrote—*An introduction to the theory of perception* (1927), wherein were integrated his views on the evolutionary development of the neurology and psychology of perception as applied to all the senses throughout the animal kingdom. In these he was greatly influenced in their respective fields by the work of Lloyd Morgan, Head, Rivers, and, above all, Sherrington. His general thesis conceived perceptual phenomena as existing on three distinct levels, distinct in their evolutionary development and morphological basis—a primitive dyscritic level upon which a more refined epicritic is superimposed, the activities of which are integrated by a syncritic mechanism subserved by the cerebral cortex. In support of this he marshalled a vast amount of evidence based largely but by no means entirely on vision. It is true that his conclusions were somewhat speculative; but they had a wide biological basis. As in the treatment of the problem of colour vision, he was loth to trust data based on introspection alone. Physical phenomena, he reasoned, could only be analyzed by the senses and interpreted in physiological terms; at a higher level physiological events were analyzable by psychological processes; but for the analysis of events on the psychological level, no measuring rods were at our disposal. Each level could be adequately understood only from the level immediately above it; if a level above the

psychological did exist we had no access to it; the analysis of perception by introspective psychology divorced from its biological basis was therefore dangerous. This neuro-psychological creed he finally summarized in a small and much less consequential monograph, *The springs of conduct* (1950), published when he was 82. In an attempt to throw light on the body-mind problem and in an endeavour to emphasize the importance of emotion as compared with reason in motivation, he urged the necessity of correlating factual knowledge derived from physiology with psychology, a process which could only be attained in his view by an increased appreciation of the functions of the central nervous system in the interpretation of human conduct.

The integration of a philosophy of perception on a biological basis was undoubtedly the main interest of Parsons's life; but this left him time for many other activities. The first of these was the industrial aspects of ophthalmology. This long occupied his attention, and he was a pioneer in the application of physiological and medical knowledge to the practical visual problems of industry and in this respect put his experience freely at the disposal of the state. Initially interested in the pathological aspects of this subject by his appointment as a member of the Glass-workers' Cataract Committee set up by the Royal Society (1906), and in its functional aspects by his appointment to the Home Office Committee on Factory Lighting (1909), and to the Departmental Committee on Sight-tests set up by the Board of Trade (1910), he spent much time visiting factories throughout the country and acquiring a first-hand knowledge of industrial conditions. With a view to the improvement of visual hygiene in industry, he helped to found the Illuminating Engineering Society, initiated to 'investigate lighting scientifically and to disseminate knowledge which might lead to improved standards in practice'. Invited to become its first president, he declined with characteristic modesty, holding that the post should properly be held by a physicist or an engineer. Instead he accepted the office of chairman of the Council, and took an active part in its affairs, read the first technical paper to the Society on 'Glare, its causes and effects', became its president in 1924, and was elected an honorary member in 1943.

In this new application of ophthalmology he had now acquired an international reputation, and he was invited to open the discussion on damage caused to the eye by undue exposure to light at the VIIth International Congress of Medicine (1913). Meantime, his work for the Home Office went on, and when, in 1920, the Departmental Committee renewed its work after the war, he was once more a member; he joined it again when it was re-constituted, shortly after the Factories Act (1937) had made it for the first time a statutory duty for 'sufficient and suitable lighting' to be provided in all factories throughout the country. In the inauguration of this desirable social improvement Parsons had played a prominent part throughout the 31 years of the committee's activity; and in 1940 he signed its fifth and last report, on which the present factory lighting regulations are based. In addition to this

major task, he served on the Committee on Eyestrain in Cinemas, appointed by the Illuminating Engineering Society (1919); on the Committees on the Causes and Prevention of Blindness set up by the Ministry of Health (1920; 1936); on the British Medical Association's Special Committee on Tests for Motor Drivers (1929-31) and on Miners' Nystagmus (1935-6; 1938-40); and, finally, on the Illumination Committee of the Department of Scientific and Industrial Research.

He also gave his services to the state in war. In the First World War he served initially, with the rank of Captain R.A.M.C., as Ophthalmic Surgeon to the 3rd London General Hospital (1916-17), and then, with the rank of Colonel A.M.S., as ophthalmic consultant to the Home Forces (1917-18). In 1919 he joined the Advisory Medical Council of the Air Ministry; and in 1922 that of the Admiralty. Just before the Second World War he became civil consultant to the Royal Air Force, and shortly thereafter joined the Flying Personnel Committee which concerned itself with all matters affecting the safety and efficiency of the R.A.F., as well as the corresponding Committee in the Army. For such public services, as well as for his scientific eminence, he had been created C.B.E. (military) in 1919, and knight bachelor in 1922.

In addition to these scientific and public activities, Parsons always participated wholeheartedly in the social and professional life of ophthalmology. He joined the Ophthalmological Society in 1900 and remained one of its staunchest supporters until age and deafness precluded his active participation in its activities. Rarely did a meeting occur in which he did not make a contribution, either presenting a paper or taking a prominent part in its discussions: to its *Transactions* he made some 140 contributions. In the ophthalmological activities of the British Medical Association and the Royal Society of Medicine he was equally assiduous. He performed the arduous task of being Chairman of the Editorial Committee of the *British Journal of Ophthalmology* for 32 years, from its foundation in 1917 to 1948. In medico-political matters he strove to maintain the professional standards of ophthalmology; he was one of the founder-members of the British Council of Ophthalmologists, remained its moving spirit and was largely responsible for its dissolution and the creation in its place of the more representative and authoritative Faculty of Ophthalmologists, which is now the co-ordinating and academic custodian of the specialty.

In international ophthalmology he also took a prominent part. At a time when deep feelings and animosities were still persisting after the First World War, together with Treacher Collins and Leslie Paton, he was largely responsible for the resumption of normal professional relationships between the belligerents. Owing to the persistent refusal of the French to meet the Germans, plans for an international meeting proved abortive in the early 1920's; but Parsons was one of the moving spirits in arranging the very successful English-speaking Congress in London in 1925, in engineering the creation of the International Council of Ophthalmology in 1927, and in

bringing to a successful consummation the thirteenth International Congress of Ophthalmology in Amsterdam in 1929—a delightful meeting at which the nations of the world resumed the continuity of their academic association. In these endeavours Parsons travelled much, particularly to America, where he proved an acceptable ambassador and speaker. With his foreign friends in many countries he maintained a close and intimate correspondence, and his house was a port of call to most ophthalmological visitors from abroad.

Of the greatest importance to ophthalmology, however, was his participation in the work of the Medical Research Council, on which he served from 1929 to 1932. A close friend of its first Secretary, Sir Walter Fletcher, and for long the chairman of its Committee on Vision, he bent all his powerful influence to stress the scientific and national importance of research in visual problems; and he succeeded. It was due to his efforts and his encouragement that a nucleus of research in this subject was kept alive in this country in the years between the wars, and he was essentially responsible for the provision of opportunities and funds for this purpose, supplementing these material endeavours by his personal interest in those whom he stimulated to undertake original work. To foster research in the specialty he brought forward in the early 1920's a scheme to amalgamate the three large ophthalmic hospitals in London—Moorfields, the Royal Westminster and the Central Eye Hospitals. That this scheme did not materialize at that time was largely due to the fact that he found himself unable to force a difficult issue in medical politics and to gather the necessary financial support; but it was in tribute to his foresight and the preliminary work he had accomplished that he was asked, at the age of 79, to open the Institute of Ophthalmology which eventually emerged as a result of this amalgamation, in 1947.

Recognition for these services, public, scientific and social, was not wanting. In 1903-4 the Royal College of Surgeons invited him to give the Arris and Gale Lectures, his subject being 'The neurology of vision'. In 1904 he received the Middlemore Prize from the British Medical Association; and again in 1914. In 1907 he received the Nettleship Gold Medal from the Ophthalmological Society of the United Kingdom for his pathological work, and in 1925 this same society gave him its highest tribute by appointing him to the Bowman Lectureship, for which he delivered a remarkable paper on 'The foundations of vision'; and in that year he was also elected its president. In 1919 he received the Doyne Medal from the Oxford Ophthalmological Congress, delivering a lecture on 'Preventive ophthalmology'. In 1921 he was elected a Fellow of the Royal Society, in the affairs of which he took much interest, serving on its Council (1926-7) and being appointed a Vice-President in 1942. He received the honorary degree of D.Sc. from his own University of Bristol (1925), and LL.D. from Edinburgh (1927), and was elected a Fellow of University College London. In 1929 he was invited to take part as the representative from Great Britain in the opening of the Wilmer Institute of Johns Hopkins University, when he delivered an address on 'Colour vision and its anomalies'; and again in 1936 he was invited to the U.S.A.

as the guest of the American Academy of Ophthalmology and Otolaryngology, when he delivered a lecture on 'Ophthalmology and research'. On this visit he also received the Howe Medal of the American Ophthalmological Society in recognition of his services to this branch of medicine. His outstanding position abroad was further recognized by his receiving the honorary membership of the American, Australian, Egyptian, Hungarian and Japanese Ophthalmological Societies, and of the American Medical Association; while his position in medicine at home was recognized by his election as President of the Royal Society of Medicine (1936-8)—the only ophthalmologist so far to occupy this position—and as an honorary Fellow in 1942. On his 80th birthday, as a token of their affection and respect, he was presented with his portrait by his colleagues in the Faculty of Ophthalmologists and the Ophthalmological Society; this portrait now hangs in the Institute of Ophthalmology. On the same occasion a special number of the *British Journal of Ophthalmology* was published in his honour consisting of contributions made by his scientific and clinical pupils and associates. This, he said, was the happiest day of his life.

In his later years Parsons had retired completely. In 1939 he vacated the large house in Queen Anne Street, London, where he had lived, practised and written for many years, lending it to the French Red Cross. He went to Leeds to live with two long-standing friends, who tended him in his later years with a deep devotion. After the war he divided his time between Yorkshire and London, taking for a time a considerable interest in ophthalmological and general scientific affairs. But deafness, an annoying tremor and cardiac troubles, disabilities strange and exasperating to a nature so forceful as his, gradually curtailed his activities; and the end, when it did come, was for him not unwelcome. His wife had died in 1911, and he left a son and a daughter.

This was indeed a full life and a proud record; but Parsons did not live for work alone. He enjoyed company and was a good host. He knew much of music and used to go to the opera, taking with him a score for study. His knowledge of literature and languages was wide; as a hobby he studied Persian, and took much interest in the art of that nation. He enjoyed golf, and travelling was a great pleasure to him; even in advanced age he found relaxation in a regular holiday cruise.

To the outside world he often appeared reserved and distant, sometimes assuming a superficial appearance of hardness and cynicism by which he defended himself from the world. Completely honest by nature, he had no place for hypocrisy or dubious compromises, and made no attempt at placating others. A shrewd judge of character, he was no respecter of persons; his standards were high, his criticism often severe, and he said what he thought without hesitation—often with a wit so mordant as to be devastating. Yet this somewhat forbidding exterior clothed a nature that was essentially shy and modest. To his patients he was the kind and sympathetic physician; and on those of a younger generation, whom he inspired to undertake research

and investigation, he lavished much kindness, always making himself accessible to them and unfailingly offering them advice, encouragement and practical assistance. Many of these, of whom I had the good fortune to be one, often guests at his home or gathering around him in scientific discussions, regarded him with almost filial affection. Rarely have an intellectual ability, a profundity of philosophical thought, and a forcefulness of character so great, been combined with a disposition so loyal and fundamentally affectionate. It was fortunate for British ophthalmology and the scientific study of vision in this country that he lived in his generation.

STEWART DUKE-ELDER

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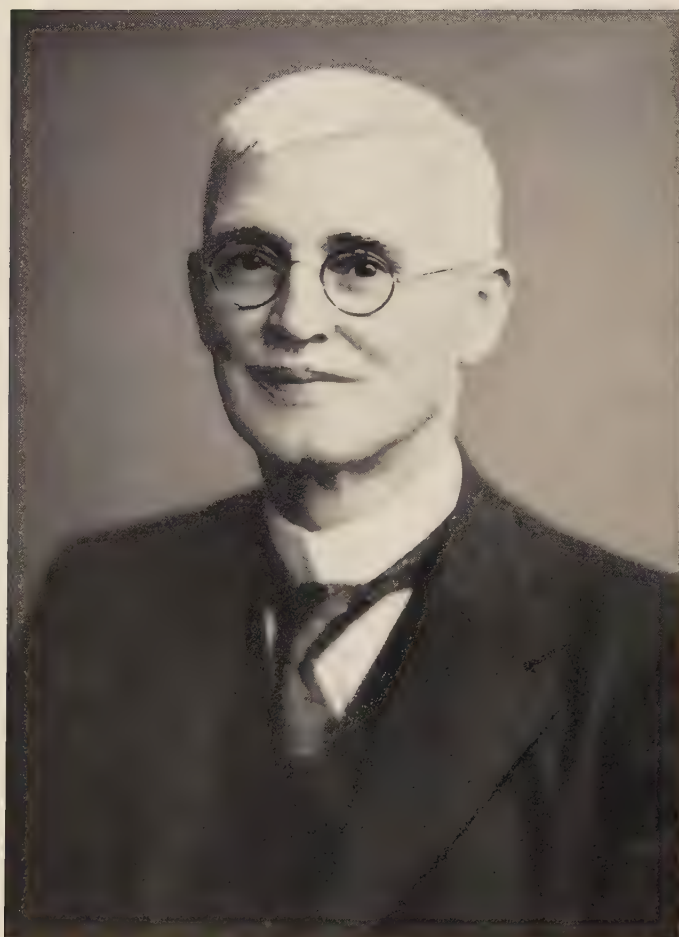
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A. J. Rowledge

ARTHUR JOHN ROWLEDGE

1876-1957

ARTHUR JOHN ROWLEDGE was born on 30 July 1876, at Peterborough.

His father had a small building business which had been inherited from his grandfather who had worked on the building of the Crystal Palace at Sydenham. He was quite good at most trades and could sketch and make plans.

His mother came from farming stock in Lincolnshire. There were three brothers and one sister in the family, Arthur John being the youngest of the four children.

In his early boyhood he used to spend a good deal of his time at his maternal grandfather's farm in Whaplode Fen, but he had to give this up when he started to attend school. His father interested him in sketching and the arts, taking him to the National Gallery. He also explained many scientific and mechanical things to him so that, for example, he knew how the steam engine worked when he was quite a small boy.

He received his schooling at St Peter's College School, Peterborough, where Mr Seabrook was Headmaster. There was also a school of science and art in the Minster yard which he attended whilst he was at the college school and also during his apprenticeship. He first studied art there, but later on, when he started work, concentrated on mechanical and scientific subjects, and in May 1891 obtained a Queen's prize in the examination of the Science Schools.

He was apprenticed to Barford & Perkins, Engineers, of Peterborough, and Mr Perkins who was then Works Manager, taught him a great deal. Although the works plant was crude, the fundamentals of many modern developments were there, particularly in the foundry and in the jig methods used.

He took a keen interest in the technical press and at that time a paper by a Scots professor on the properties of similar structures and machines started a line of study which was to be of the greatest use to him in later years.

On the completion of his apprenticeship in 1898, he went as a draughtsman to R. Hoe & Co., the London branch of the American printing machine makers. This was his first introduction to intricate mechanism and made quite an impression on him, as he would often quote in later life the practices used in printing machinery.

In 1900 he went for a short time to Easton, Anderson and Goolden at Erith, where he obtained experience of what he felt was real engineering, and in the next year he obtained a post as designer with D. Napier & Sons,

who were then building motor cars in a factory near Waterloo Station. He was with them from 1901 to 1904 and during this period the Napier car won the Gordon Bennett road race. They also built what was, perhaps, the first six cylinder motor car.

In 1905 he joined the Wolseley Company and in June of that year he married Alice, daughter of H. H. Blincko, Esq., and Louisa Blincko, at St Philip's Church, Kennington. There were three children of the marriage: Jeanie Louise, born 23 January 1907—studied art and obtained her A.R.C.A. diploma; John William, born 17 August 1908—now a chartered accountant; Donald Henry, born 28 September 1911—studied architecture and qualified A.R.I.B.A.

There is little record of what he did at the Wolseley Company but, over a large part of the time he was there, he held the position of Chief Designer.

In November 1913 he went back to the Napier Company as Chief Draughtsman and later became Chief Designer. He was responsible for the design of a number of Napier cars which could be regarded as the forerunners of the modern motor car.

During World War I the Napier Company was initially engaged in building aircraft engines which had been designed in the Royal Aircraft Factory and also by the Sunbeam Company. As a result of experience gained in building these engines, Rowledge commenced in 1916 the design of the famous 'Lion' engine in which the cylinders were water-cooled and arranged in three banks in a broad arrow formation. He had arrived at this configuration as the result of a study of the requirements of the military fighter of that day, and this also led to the use of a propeller driven by a spur reduction gear and placed roughly at the centre of area of the frontal aspect of the engine, in order to give a good faired shape to the nose of the aircraft. We see here the first manifestation of his belief in the necessity to treat the engine-aircraft problem as an entity, a practice which he was to follow with great thoroughness throughout his later designs.

Development of this engine eventually led in 1918 to the Lion series II 450 b.h.p. engine which, although it arrived too late to see much war service, was to become one of the standard engines of the Royal Air Force for many years. The Lion was the first to reach a height of six miles; winner of the 1919 Aerial Derby race; first to fly non-stop from London to Madrid, covering the 900 miles in a record $7\frac{3}{4}$ hours; first to carry a load of $1\frac{1}{2}$ tons to 13 999 ft.; and holder of 24 British records for speed, duration and load-carrying. It was also the lightest aero-engine for h.p. of any previously produced. For his work on these projects he was awarded the M.B.E. at the end of World War I.

To satisfy the heavy bomber requirements of that day he designed a further engine in 1919 known as the 'Cub'. This had 16 cylinders arranged in four banks in an X formation and is worthy of specific mention because it was the first aircraft engine to develop 1000 b.h.p. in a single unit. This engine, because of the changing circumstances and recession after World War I, was not produced beyond the first six prototypes, but Rowledge was to

return to this configuration in later years in the Rolls-Royce 'Exe' engine.

In 1921 he resigned from the Napier Company and joined Rolls-Royce Limited as Chief Assistant to Mr F. H. Royce. By this time the large proportion of the firm's man-power engaged on aero-engine work during the 1914-1918 war had shrunk to relatively small numbers and Rowledge was brought in as the first step towards a resuscitation of effort in this field.

The political and economic background which was to be his greatest obstacle over the next few years was only just taking shape. Economy in government circles was the order of the day and the prospect of adequate government support for the aircraft and aero-engine industries was far from bright.

He was now 45 years of age and in his prime. His tall loose-limbed figure was soon well known throughout the Company. Never in a hurry, he yet moved quickly, and what he saw he remembered. He assessed the equipment available in the factory, the potential of the organization and adapted his designs to use both to their fullest capacity. Where shortages were apparent he would good-humouredly suggest expediciencies.

His first job was to assess the development potentialities of the existing Rolls-Royce Condor and Eagle aero-engines. At the end of a few months he emerged with a project, that of redesigning the Condor engine to make it suitable for both military and civil applications. He obtained a small staff of three designers to put his ideas into practice and into them, as indeed into all who were afterwards in intimate contact with him, he implanted the seeds of his own genius which ultimately again raised the Rolls-Royce aero-engine to its World War I pre-eminence. The immediate target he set was to raise the brake horse power of the Condor from 540 to 650, to reduce the weight drastically, to reduce the overall dimensions and to improve the reliability.

Basic knowledge was necessary to provide progress in many directions and he forthwith set out by rig testing and other means to establish without doubt that his estimates and claims for the new and improved features of the engine were capable of attainment.

The following year saw the Condor III design completed and the first engine built. The objectives of increased power, reduced weight and size were reached and development continued for reliability under his guiding hand. Much of the early development work on lead bronze bearings was carried out on this engine, a material which was to replace white metal on all the later engines to be developed by the Company.

The Condor III was eventually to engine the famous Hawker 'Horsley' bomber which was produced in what was, for those times, fairly large numbers. This aircraft came very near to establishing a world long distance flight record.

Two other items are worthy of mention during this period. In order to improve take-off and cruise performance, he designed a two-speed propeller reduction gear for the Eagle engine. This was a remarkable achievement as

it was accomplished with little extra complication and increase in weight over the existing gear. Although the gear was made and flown and did the job that it was intended to do with eminent satisfaction, it came at a time when the requirements of supercharging were beginning to force the development of the variable pitch airscrew and, once this was established, the need for a two-speed gear did not again arise until piston-engined fighter aircraft were reaching speeds approaching 500 m.p.h.

The other item was the application of an exhaust driven supercharger to the revised Condor engine in order to improve its performance at high altitudes. This was unique at this time in that a two-stage supercharger unit was used in order to keep down the overall dimensions and so enable it to be installed in the vee between the cylinder banks.

During this period he also found time to devote attention to the braking problem on the Rolls-Royce motor car. Four-wheel brakes were being introduced and, in order to provide the deceleration required on a fast and heavy car with light pedal pressures and small pedal movement, it became necessary to introduce servo assistance. He produced a design for this using a low speed friction clutch driven from the gearbox, the design being such that braking loads were exactly proportional to pedal pressure and were equally effective in reverse. This basic design is still in use on the Rolls-Royce motor car today and no greater tribute to its effectiveness can be made than this statement.

The effort to develop the existing series of aero-engines showed up some inherent weaknesses in the type of engine using separate steel jacketed cylinders. At about this time the American Curtiss engine, which was a water-cooled twelve cylinder of vee formation, using monoblock cylinder castings in light alloy, was meeting with considerable success.

In order to meet this competition a design for a completely new twelve cylinder 5 in.-bore engine, known as the 'F' engine, using monoblock cylinders was put in hand. This engine was much smaller in overall dimensions than the Eagle engine which it replaced and was more powerful. It had a beauty and simplicity of line which exemplified his saying: 'If it looks right, it is right.' This engine eventually became known as the Kestrel and was among the most successful of its time. It was extensively used in the Royal Air Force in a series of well-known Hawker fighter biplanes.

In pursuance of his practice of looking at the aircraft-engine problem as a whole, he gave considerable attention to improved engine cooling methods which would at the same time result in reduced radiator area and drag. This resulted in the evaporative cooling system which, although it never found its way into extensive Service use because of certain difficulties, nevertheless paved the way for the use of increased coolant temperatures and special coolants, both of which were to become common features of all future liquid cooled engines.

He also paid considerable attention to supercharging and produced a design for a gear driven supercharger for the Kestrel engine which was

eminently successful and which formed the design basis of what was to become an integral part of all future Rolls-Royce piston engines.

During 1927 the need for a more powerful engine for flying boat applications led to a design for a 6 in.-bore engine generally following the lines of the Kestrel. This engine was known as the 'H' or Buzzard engine and became the basis of the 'R' engine of Schneider Trophy fame, the development of which over the next few years was to become Rowledge's chief pre-occupation.

Following on the decision to enter a Supermarine aircraft for the Schneider Trophy contests to be held in 1929, work started on the 'R' engine in 1928. Co-operation with Royce was maintained at frequent intervals and the combined efforts of Rowledge's small design staff and part of Royce's design staff resulted in the design being completed and the first engine ready for test in an incredibly short period.

During the design period and indeed over the whole period of the exercise there was very close co-operation between Rowledge and Mitchell, the designer of the Supermarine contender for the trophy, with the result that the engine was meticulously tailored to suit the aircraft. For the first time the engine intake, at Ellor's suggestion, was designed to take advantage of the speed of the aircraft to boost the engine output by ram effects, a feature to be incorporated later on in practically every aircraft engine. Something like double the power output of the Buzzard was the target.

Under Rowledge's guidance, development testing of existing engine components had been initiated early so that the designers were able to incorporate features found necessary by the increased loading. Crankshafts broke, connecting rods bent, cylinders and valves gave trouble and pistons disintegrated, but Rowledge showed his calibre and capacity by never being pushed off his balance however catastrophic the failure. He just guided and inspired his team of which he said: 'Nowhere in the world was there such a band of workers and enthusiasts.' A number of new problems relating to fuels and lubrication had to be solved and he brought in experts from the oil companies to deal with these.

The result was a triumph of performance, an engine of 1900 b.h.p., 1530 lb. weight (0.805 lb./b.h.p.) or 0.75 lb. b.h.p. for the maximum power of 2040 b.h.p. achieved on the test bed. The Schneider Trophy contest was won on 7 September 1929, at a speed of 328.63 m.p.h. and the world speed record obtained on 12 September 1930, at 357.7 m.p.h. Britain had at long last regained world leadership in the aircraft sphere, and tributes to Royce and Mitchell were loud and long. Of Rowledge, it is appropriate to quote here an extract from *The Aeroplane* of 18 September 1929.

'But there is one other man whose name, so far as one can remember, has not been mentioned in the praises which have been poured so deservedly on all who have had a hand in our victory, that is Mr A. J. Rowledge the Chief of the Design Staff of Rolls-Royce Ltd. He cannot be allowed to pass unhonoured and unsung.

'Mr Rowledge is in the curious position of having been intimately concerned with the designs of both the Napier "Lion" and the new Rolls-Royce engine.

'He is such a retiring individual and so seldom appears in public that he is nothing like as well-known as he deserves to be. But on such an occasion as this he deserves full acknowledgment for his share in developing what are probably the two finest water-cooled engines in the world.'

The year 1931 saw Rowledge at the peak of his career. His Condor III engine was doing good work in the Iris flying boat and other aircraft; the Kestrel was setting new standards in aircraft like the Hawker 'Fury', and the pre-eminence of the designs to which he had applied his genius was recognized the world over.

The government had decided on economy grounds not to participate in the Schneider Trophy race for that year. However, participation was made possible by a private donation of £100 000 from Lady Houston and in January 1931 a decision was made to enter a development of the same basic aircraft and engine which had won the trophy in 1929. This left little time in which to prepare for the race. However, by working 'round the clock' the engine power was boosted to 2350 b.h.p., and on the day the Schneider Trophy was won outright for Britain at a speed of 340·8 m.p.h. A little later on, 29 September, with the same aircraft and engine, the world speed record of 407·5 m.p.h. was established. The highest recorded power during the 'R' engine development in 1931 was 2783 b.h.p.

His work on this project was recognized in 1931 by the presentation to him, by the Institution of Automobile Engineers of the Institution Medal 'for the prominent part he took in the development of the design of the "R" engine fitted to the winner of the Schneider Trophy in 1931', and in 1932 by the presentation, from the same Institution, of the Simms Gold Medal 'for his work on design and research of outstanding merit in connexion with internal combustion engines as applied to high speed aircraft.' In 1936 he was presented with the British Silver Medal of The Royal Aeronautical Society 'for your scientific achievements in the development of aircraft engines'.

In after years the 'R' engine formed the basis of the Griffon engine which was eventually to power the later marks of Supermarine Spitfire fighters during World War II and the Avro Shackleton marine reconnaissance aircraft. However, it was considered at that time too large for the next generation of fighters and in 1932 Rowledge turned his attention to an intermediate size engine of 5·4 in. bore. This followed the same lines as the Kestrel and the 'R' engine except that, in order to improve the pilot's view in fighter aircraft, he inverted the engine. Although this proposal did not receive much favour from the British aircraft designers, it is interesting to note that the inverted arrangement was used by the Germans for both the Junkers and Daimler-Benz water-cooled twelve-cylinder vee engines during World War II. This engine subsequently reverted to the upright configuration and in this form became the famous Merlin.

The exacting demands of the development of the 'R' engine over the previous years had taken its toll and soon after Sir Henry Royce died in 1933, Rowledge had a serious illness and was absent from his work for some time. On his return after his recovery, he was appointed Chief Consultant to the Company, and in this capacity he began to look round as to what the next aero engine development could be.

He had been a consistent champion of the water-cooled engine over the years for a number of reasons, but a survey of the achievements and potentialities of the air-cooled engine at that date, together with certain difficulties which had never been completely solved in the water-cooled engine, convinced him that air cooling properly and efficiently carried out offered greater advantages. In addition a further weakness of the poppet valve type engine had been shown up in failure of the exhaust valve, particularly with the more general use of special fuels to deal with detonation. He had for some time been watching keenly the development of the single sleeve valve on the Bristol engines and the achievement here led him to recommend the use of this type of valve.

After careful consideration of all possible configurations he recommended an engine of X form with 24 cylinders arranged in four banks of six, directly air-cooled and using single sleeve valves. The air entry duct for cooling purposes was between the vee of the lower cylinder banks, the air cooling outlet being taken from the side vees which contained the exhaust pipes. He retained the use of an offset spur propeller reduction gear which brought the cooling air duct entry in line with the position of maximum slipstream velocity from the propeller and made the cooling effect more closely related to power output.

His recommendations were accepted, and a separate small design and development team was established. Designs were put in hand for a 1000 h.p. $4\frac{1}{4}$ in.-bore $\times$ 4 in. stroke engine known as the 'Exe' and the first prototype was built in 1936.

A number of difficult mechanical problems had to be solved on such an engine, for example, the design of the big end which had to carry four connecting rods; in accordance with previous practice, he designed a number of test rigs in order to solve the various mechanical problems cheaply and quickly.

He also evolved a further improved cooling scheme for the cylinders and with this and other improvements, the engine was developed to be an extremely reliable and smooth running power unit. A prototype engine was installed in a Fairey 'Battle' at the Company's flight establishment at Hucknall and proved to be so trouble free that it was used as an aerial taxi for a period of two years.

The 'Exe' engine was roughly of the same power output as the Merlin which was being concurrently developed by the Company, and at the outbreak of hostilities in World War II in 1939 the difficult decision had to be made as to whether the development of the 'Exe' should proceed. In view of

the aircraft commitments tied up with the Merlin and other considerations, the decision was made to drop work on the 'Exe'. This type of engine was in fact revived after World War II in a scaled-up form as a transport aircraft engine. It was known as the Pennine, designs were completed and a few prototype engines were built and tested. However, at that time the gas turbine was beginning to evolve in a practical form as an aircraft power plant, and the decision was made to concentrate the Company's activities in this direction, with the result that work was stopped on the Pennine.

Rowledge retired in January 1945, at the age of 69. He lived in quiet retirement in Derby for many years but increasing deafness was a great handicap. He died on 12 December 1957, after a long illness.

Rowledge was essentially a kindly, modest and quiet, self-effacing man, neither seeking nor welcoming publicity. In consequence he had no prominent activity apart from his chosen profession.

He took a great interest in the training and the problems facing young engineers and for some years served on the Rolls-Royce Selection Board for apprentices. He was always very willing to help the young engineer to many of whom he became a valued friend and counsellor. This educational interest extended outside his Company and he was a member of the Derby Corporation Higher Education Committee with special reference to the Technical College.

For many years he held office in the Derby Branch of the Institution of Automobile Engineers (later incorporated into the Institution of Mechanical Engineers) and it is very largely due to his efforts that the Branch became fully established and maintained an activity comparable with any other provincial centre.

He was a Fellow of The Royal Aeronautical Society and a Foreign Fellow of the Institute of Aeronautical Sciences N.Y.

He served on two sub-committees of the Aeronautical Research Committee and on several British Engineering Standards Association Committees.

Rowledge had an interest in Local Government Health matters and was a member of the Management Committee of the Derbyshire Royal Infirmary. This interest was actively shared by Mrs Rowledge who did much good work in visiting the sick in local hospitals.

His early interest in art and architecture continued throughout his life and this no doubt influenced the careers chosen by two of his children. He himself was fond of painting in water colours.

He was a music lover and theatre goer. The Gilbert and Sullivan light operas in particular afforded him great pleasure. He read extensively and enjoyed poetry.

Moderately religious he attended Derby Cathedral regularly with his family. He was essentially a home lover being devoted to his wife and children.

With this background of family life it was not unexpected that he should be a kindly, understanding man, particularly open minded and willing to

listen to and appreciate the point of view of the succeeding generation. Always ready to give advice based on his own experience, by constant observation he kept himself fully up to date with changing conditions and views.

A. A. RUBBRA



F. Pimon

FRANZ EUGEN SIMON

1893-1956

SIR FRANCIS SIMON, physicist, great thermodynamician, and for many years universally recognized leader and doyen of low-temperature physics, died on 31 October 1956 in Oxford.

His purely scientific work, his contributions to the development of atomic energy and his forcibly expressed views about the impact of science and technology on public affairs were all closely interwoven. His many scientific interests and preoccupations permeated his whole life and coloured his relations with friends, colleagues and pupils. Nevertheless, a separation, however artificial, of the various aspects of Simon's career has been attempted in this memoir which is accordingly divided into the following sections:

I. LIFE, which contains only brief references to Simon's work.

II. WORK.

- (1) *Scientific research*. A survey, under several sub-headings, of Simon's researches which covered the whole field of low-temperature physics.
- (2) *Atomic energy*. A brief account of Simon's contribution to this subject, chiefly during the 1939-45 war.
- (3) *Science, industry and public affairs*. A summary of Simon's views on various topics such as scientific and technological education, fuel and power policy, and the future of nuclear power.

III. PERSONALITY AND INFLUENCE.

I. LIFE

Franz Eugen Simon was born on 2 July 1893 in Berlin, where his father was a wealthy merchant. He was the only son, with an older and a younger sister. There is no record of any scientists or men of learning among his immediate ancestors, but it is of interest that his ancestry on his mother's side has been traced back to the brother of Moses Mendelssohn, the eighteenth-century Jewish philosopher. One of his first cousins is K. Mendelssohn, the well-known low-temperature physicist.

In 1903 he entered the Kaiser Friedrich Reform Gymnasium in Berlin. This school had two 'streams', one classical and the other 'modern', with the emphasis on modern languages and science. Simon's predilection for science showed itself at this time but, on the insistence of his grandfather, he was made to enter the 'classical stream' and this enforced study of Latin and

Greek at the expense of science probably coloured his later views on the relative merits of classical as opposed to scientific education. He spent several of his school holidays in England and Scotland in order to learn English. At school he devoted as much of his time as possible to science and showed particular aptitude for physics and mathematics. Simon was barely 14 when an old friend of the family, the distinguished physiological chemist, Professor L. Michaelis (later of the Rockefeller Institute of Medicine in New York) noticed his unusual talent and encouraged him to consider physics as a profession. Simon accepted this advice with enthusiasm but there was a strong family opposition to be overcome. In the Germany of those days a physicist's career did not appear financially promising, or even secure, and Simon's father would refer to his son's intended profession as 'Brotlose Kunst'. Michaelis finally said to Simon's father that 'If a man as rich as you would not allow his son to study physics, who should?' and that settled matters. After leaving the Gymnasium in the spring of 1912 Simon entered the University to study physics, chemistry and mathematics.

He first spent a year at Munich where Sommerfeld was one of his teachers and then went on to Göttingen where he studied under Tamman and Courant among others. It should perhaps be mentioned that it was then the general practice in Germany for students to spend their first two years at a number of different universities. The absence of any examination before the D.Phil. examination made this system possible. Its obvious advantage was that students had the opportunity of getting to know the great teachers of their day.

In the autumn of 1913 Simon was called up for his one year of compulsory military service and he was still serving on the outbreak of war. During the 1914-18 war he served in the Field Artillery with the rank of Lieutenant mainly on the Western front. He had the unenviable distinction of being one of the earliest poison gas casualties of the war; he was also twice wounded, the second time only two days before the Armistice and so severely that it was not until the spring of 1919 that he was released from a military hospital. He was awarded the *Eiserne Kreuz, Erste Klasse* (Iron Cross, 1st Class), a decoration given only for personal bravery and somewhat similar in character to the British Military Cross. Simon, usually a most communicative man, hardly ever talked about his war experiences and did not regard his decoration with any particular pride. Later in life he used to refer to it rather as a joke, pointing out that he was probably the only man with 'E.K. Erste Klasse' and a C.B.E.

At Easter 1919, after a break of six years, Simon resumed his studies, now at the University of Berlin. He attended lectures by Planck, von Laue, Haber and Nernst, to name only those whose influence he himself regarded as the most important, and in January 1920 he began work on a D.Phil. thesis under Nernst, then Director of the *Physikalisch Chemisches Institut* of the University. His subject was a study of specific heats at low temperatures (that is, down to the temperature of liquid hydrogen). Judging from

the wealth of material contained in his dissertation (published in a shortened version in the *Annalen der Physik*) and from the fact that he completed it in barely 18 months he must have been working with untiring energy, as if to make up for the six lost years. Many of the problems that were to occupy him during the following decade are tackled in this thesis. He studied the specific heat of ammonium chloride and discovered the λ -type specific heat anomaly. Other measurements dealt with the specific heats of undercooled liquids, of glasses and of various crystalline substances. Finally, the results were discussed with particular reference to the Nernst heat theorem and the theories of specific heats.

Simon obtained his D.Phil. in December 1921, having passed his examinations in experimental physics, theoretical physics, chemistry and philosophy. In 1922 he was appointed Nernst's assistant and in the same year he married Charlotte Münchhausen whom he had met two years before. It was a profoundly happy marriage and his domestic happiness with his wife and two daughters coloured his whole life and served as a safe anchor through all the troubles of subsequent years. His wife was not a scientist and Simon never tried to interest her in the details of his work. On the contrary, it was he who found recreation and much needed relief from his exacting scientific work by sharing his wife's interest in music and the visual arts.

Mention may here be made of his deep affection for his mother who spent the later years of her life in England and died at the age of 80 in 1946, and for his two sisters. His elder sister studied medicine (they spent one semester together at Munich) but gave up her studies when she married Dr L. Frank who was to become a well-known physician. They settled in London in 1936 and it was with Simon's encouragement that their younger son, S. F. G. Frank, became a physicist. Simon's younger sister studied history of art and became a curator of the former Imperial Palaces in Berlin. In 1933 she and her husband, Dr A. Henschel, emigrated to Palestine where, as keeper at the Rockefeller Museum in Jerusalem, she did archaeological work until her untimely death in 1946.

From a scientific point of view Simon's Berlin period was most fruitful and successful. He stayed on at the Physikalisch Chemisches Institut when Bodenstein succeeded Nernst as Director; in 1924 he became Privatdozent and in 1927 Ausserordentlicher Professor (roughly equivalent to a Reader in Britain or an Associate Professor in the United States). During his ten years in Berlin he built up a low-temperature department which, though modest in physical size, was impressive in achievement. By 1929 there were about ten 'Doktorands' and usually a couple of 'Assistents' working either directly under his supervision or in close co-operation with him.

The laboratory itself consisted of two rooms measuring about 15 ft. by 60 ft. in all and an additional small office containing Simon's desk, a bookshelf and a sofa on which he took his regular siesta (a habit he retained throughout his life) and which could be used by anyone for brief rests during night-long experiments. The place was overcrowded and always busy, day

and night, semester or vacation so that it was almost impossible to keep the laboratory neat and tidy, much to Simon's chagrin. He disliked untidiness and always tried to coax people into giving the laboratory a more respectable appearance. It was all done in good humour and Simon was able to laugh heartily when, at a Christmas party his students put on a sketch which started with the cry 'Tidy up, tidy up! Visitors in sight.'

Among the most important achievements of the Berlin period were the solidification of helium and of other gases by high pressures and the discovery of the specific heat anomaly in solid orthohydrogen. Less spectacular but of equal value were the large number of publications on specific heats, the Nernst heat theorem, the properties of adsorbed gases and crystal structure studies by X-rays.

Such an ambitious programme needed a steady supply of liquid hydrogen. Nernst's old liquefier, which Simon had used for his D.Phil. work, was much too small and for several years the liquid hydrogen was obtained from W. Meissner at the Physikalisch Technische Reichsanstalt. By 1930, however, a new hydrogen liquefier built to Simon's own design was installed in the Physikalisch Chemisches Institut. A number of replicas of that apparatus, built privately by the head mechanic of the Institut, were subsequently bought by laboratories in Germany and elsewhere. When F. A. Lindemann (later Lord Cherwell) decided to initiate low-temperature work at the Clarendon Laboratory he turned to Simon, whom he had previously met, and in 1930 one of these hydrogen liquefiers was installed in Oxford.

Another important cryogenic development of the Berlin period was the liquefaction of helium on a small but nevertheless useful scale by the desorption method. In this way Simon's laboratory became the fourth institution at which experiments down to the temperature of helium could be carried out, the others at that time being Leiden, Toronto and the Physikalisch Technische Reichsanstalt in Berlin.

By the end of the 1920's Simon had become a well-known and respected figure in the world of physics. He attended many scientific meetings throughout Europe and in the summer of 1930, accompanied by his wife, he went to a meeting in Odessa, returning to Berlin via Moscow and Leningrad. This was his only visit to Russia. It was during these years too that he became acquainted with numerous colleagues from all over the world and in many cases acquaintances springing from common scientific interests later ripened into real friendship.

Early in 1931 Simon was appointed to the Chair of Physical Chemistry at the Technische Hochschule of Breslau in succession to A. Eucken, who had gone to Göttingen. It was not an easy decision to leave Berlin which in those days was one of the scientific centres of the world. On the other hand there were some attractions in going to Breslau such as the prestige of the position of an 'Ordinarius' and the prospect of building up a larger and better equipped low-temperature laboratory than had been his in Berlin. In the event this latter hope was never realized and the Breslau period became a

brief interlude between the brilliant beginnings in Berlin and the gradual but rich achievement in Oxford.

About the time of his move to Breslau Simon was invited by G. N. Lewis, then chairman of the Chemistry Department at Berkeley, California, to spend the spring semester of 1932 there. He was granted leave of absence from Breslau and set off at Christmas 1931 on this first of many visits to the United States, a visit that was both enjoyable and fruitful. As on later trips, he made many new friends, of whom Professor Joel H. Hildebrand of Berkeley should be mentioned particularly. Hildebrand had met Simon in 1930 and was so much impressed by his ability that he used his influence to get Simon invited to Berkeley.

On the scientific side, it was in Berkeley that he conceived and executed his elegant idea of liquefying helium by a single-stroke adiabatic expansion and that he carried out, using his new apparatus, lecture demonstrations on superconductivity. The expansion method played an important part in spreading low-temperature research and it is interesting to note that one of the first Simon expansion liquefiers ever to be built was used for liquefying helium at the Clarendon Laboratory. Lindemann, having installed a hydrogen liquefier some eighteen months earlier now wanted to start, on a modest scale, work at helium temperatures. One of the expansion liquefiers built in Breslau soon after Simon's return was taken by Mendelssohn, at the time Simon's senior collaborator, to Oxford and there put into operation early in January 1933.

The conditions and circumstances after Simon's return to Breslau in the summer of 1932 were not conducive to intense creative work. He became Dean of the Faculty of Chemistry and Mining and as such had many routine administrative duties; in addition he had to deal with the problems arising out of the merging of the University and the Technische Hochschule. More important was the rapid deterioration of the political situation with the rise of Nazism and of anti-semitism. Many of Simon's colleagues, both in Germany and elsewhere, regarded Nazism as a passing phase and Hitler as a puppet in the hands of the Reichswehr and the great industrialists, but he had no such illusions. He also knew from personal experience that anti-semitism of a less aggressive form existed in Germany before Hitler, not only under the Kaiser but even under the Weimar Republic. He found, for instance, both during his Army Service and in his University career that, as regards promotion, his being a Jew was definitely a handicap.

When in January 1933 Hitler became Reichskanzler, Simon realized that he would have to leave Germany for the sake of his family and of his work. It is true that as a 'Frontkämpfer' of World War I he was exempt from the decrees dismissing Jews from university posts, so that his own position was not immediately threatened. But he correctly foresaw the ghastly things that were to come. In June 1933 he sent in his resignation and gladly accepted Lindemann's invitation to come to Oxford. As mentioned previously the Clarendon Laboratory already had cryogenic facilities of Simon's own

design and although there was no regular university position for him, Imperial Chemical Industries, thanks mainly to Lindemann's influence, provided research grants to enable refugee scientists from Germany to work in Britain. In addition to Simon three other low-temperature physicists, K. Mendelssohn, N. Kurti and H. London, all from Breslau, came to the Clarendon on similar grants.

After a brief preliminary visit to England Simon left Germany for good in August 1933 together with his family, and settled in Oxford which was to become his home until his death 23 years later. He left Germany without regret. His chief aim in emigrating was to give his young daughters (then aged 5 and 7) a chance of growing up in peaceful surroundings, away from the hatred and violence of Hitler's Germany. The warm and helpful welcome that the Simons, in common with other emigrés, found in Oxford was a reassuring sign.

The Clarendon Laboratory of those days was a small place and his work there had to start on a modest scale. To begin with he had only one experimental room and funds for research were so low that had he not managed to bring out of Germany some of his own apparatus and equipment, his researches during the first few years would have been seriously impeded. But the lack of material facilities was amply compensated by the friendship and encouragement of his new colleagues.

Among his many friends in Oxford scientific circles there were two with whom he remained in almost daily contact throughout the ensuing years and who should be mentioned by name: F. A. Lindemann and T. C. Keeley, the Clarendon Laboratory's senior demonstrator. Lindemann having helped Simon to settle in Oxford did much to further his work; Simon, besides acknowledging this support, and the generous spirit in which it was given, always admired Lindemann's brilliant intellect and enjoyed his stimulating company. In Keeley Simon found a steadfast friend whose loyalty and help in many difficult situations he deeply valued.

Simon threw himself with great energy into the work at the Clarendon and could regularly be seen at his experiments till the early hours of the morning. The main lines of research that he and his few collaborators and research students pursued between 1933 and the outbreak of the war were: magnetic cooling and investigations below 1 °K, the properties of liquid helium and, finally, specific heats.

The work on magnetic cooling was somewhat handicapped by the fact that no strong magnetic fields were at that time available at the Clarendon Laboratory. Thanks to the helpful interest of A. Cotton, director of the famous Magnet Laboratory in Bellevue, near Paris, it became possible to carry out some adiabatic demagnetization work with the intense fields of the Bellevue magnet. Between 1935 and 1938 Simon, with various colleagues, spent six one-month periods in Bellevue. His apparatus with its built-in expansion helium liquefier, was brought over from Oxford and thus Bellevue became yet another laboratory in which the first liquid helium was

associated with Simon's name. During these Paris visits Simon got to know many of his French colleagues and from then onwards he felt quite at home in the scientific life of Paris.

Although initially Simon had no University position, he was made an M.A. soon after his arrival in Oxford and became a member of the Senior Common Room of Balliol. He owed this first introduction to Oxford College life to the then Senior Fellow of Balliol, Dr Cyril Bailey, the distinguished classical scholar whom he had met a year earlier in Berkeley.

In 1935 Simon succeeded Dr A. C. Egerton (later Sir Alfred Egerton) as Reader in Thermodynamics and to that extent his position in Oxford became a permanent one. But shortage of laboratory space and of funds still remained serious and certainly up to 1936 he wondered whether his position in Oxford would allow for a full development of his scientific potentialities. However, the situation gradually improved and when a new building for the Clarendon Laboratory was decided upon Simon made up his mind to stay.

With the outbreak of war in 1939 low-temperature work in Oxford, as elsewhere, stopped. Simon had become a naturalized British subject late in 1938 and eagerly offered his services to contribute to the war effort; these were not at first accepted for, in the first year of the war the Government was still reluctant to entrust secret scientific work to ex-enemy aliens. With low-temperature work at a standstill and no possibilities of doing 'official' war work Simon, together with other refugee scientists, became interested in the problem of the atomic bomb before it had become an officially recognized war project. Their efforts received official sanction only in the summer of 1940 when the Government eventually decided to embark on work directed towards an atomic bomb. This incidentally is the explanation of the paradoxical fact that one of the most vital and most secret war projects was, in the early days, run primarily by foreign-born scientists.

His wife and children were evacuated to Canada in the early summer of 1940 and Simon now devoted literally all his time and energy to his work. In a very short time he became an important figure in the British 'Tube alloys' project. He was mainly concerned with the separation of the uranium isotopes by gaseous diffusion, and was in charge of an important part of the work on diffusion membranes which was carried out under his general direction in the Clarendon Laboratory and in a number of other places. He also sat on various committees concerned with the project and during the war made several visits to the U.S.A.

In 1941 he was elected a Fellow of the Royal Society. In 1945 he became a Student of Christ Church and in the same year was given the title of Professor. Shortly afterwards the University, recognizing Simon's unique position as one of the very few great thermodynamicians, created for him personally the Chair of Thermodynamics. He was made a C.B.E. in 1946 in recognition of his work on the 'Tube alloys' project.

During the war Simon's work brought him in touch with many indus-

trialists, politicians and civil servants and as a result he developed a keen interest in the wider political and social aspects of science and technology. The opinions he formed he now expressed freely and often forcibly, in contrast to his custom of earlier years; when he first came to England he considered himself a guest in this country and therefore in no position to be critical, particularly where politics were concerned.

From 1948 till 1951 he was scientific correspondent of the *Financial Times* and his articles in that paper and in other publications brought his views to a wide audience. He rapidly became a well-known figure whose advice was often sought but, as he would plaintively remark, not often heeded. He retained his connexion with the Atomic Energy Authority and served on many of its committees. He was a member of the Research Council of the British Electricity Authority and during the years 1948 to 1950 served on the Council of the Royal Society. He was Chairman of the Commission of Very Low Temperatures of the International Union of Pure and Applied Physics and in that capacity played an important part in organizing the Biennial International Low-Temperature Conferences. He was also Chairman of the First Commission of the International Institute of Refrigeration.

But the prosperity of his laboratory and its scientific work came always first among all his preoccupations. The move to the new Clarendon Laboratory took place at the end of 1939 and for the duration of the war all low-temperature equipment had to be stored away. It was not until 1946 that work on the cryogenic installations began and, since much of the equipment had to be newly built, it took several years to get the work going properly. By 1950, however, the low-temperature department of the Clarendon Laboratory was thriving and had become one of the largest and most important low-temperature schools in the world. About a third of the Laboratory was devoted to problems of cryophysics and the total number of research workers amounted to about 30.

With all his other commitments Simon could not keep an eye on all the work that was going on but he still took a detailed interest in those investigations that were close to his heart. He initiated the work on heat conductivity of crystals at low temperatures; experiments with very high pressures and on the melting curve as well as on specific heats were continued. Finally, two entirely new projects, nuclear orientation and nuclear cooling, both of which he had been planning and preparing for years, were successfully started.

He received many honours. In 1948 he was awarded the Rumford Medal of the Royal Society. In 1950 he was chosen as the first recipient of the Kamerlingh Onnes Gold Medal, established by the Dutch Institute of Refrigeration. He received the Linde Medal of the 'Deutscher Kälteverein' in 1952 and in the same year became an Honorary Foreign Member of the American Academy of Arts and Sciences. In 1955 he was knighted in recognition of both his public services and his scientific achievements.

On Lord Cherwell's retirement Simon, at the age of 62, was elected to

succeed him as Dr Lee's Professor of Experimental Philosophy and head of the Clarendon Laboratory. The prospect of this position which he was to enter on 1 October 1956 gave Simon great satisfaction and he was looking forward to leading the large and flourishing laboratory, to whose growing reputation and success he himself had contributed so much.

In the summer of 1956 he was taken ill with coronary heart disease, a condition which had first shown itself 10 years earlier. The seriousness of his illness was known neither to his friends and colleagues, nor, fortunately, to him. During his slow, apparent recovery he was actively planning the future of the Clarendon Laboratory and, soon after the date of his appointment, he began to spend a few hours a day in the laboratory. Then, at the end of October, he had a sudden relapse and died on 31 October 1956.

His death at the height of his career, when his wisdom and life-long experience could have been put to such effective use came as a painful shock. The moving tributes in the daily press by Lord Cherwell, Sir John Cockroft, Mr Michael Perrin, to mention some, and the messages of sympathy that reached Lady Simon from all over the world gave proof of the admiration and affection he inspired in his colleagues and friends.

II. WORK

1. *Scientific research*

No attempt has been made to describe in detail Simon's scientific researches. Only general outlines and selected examples are given, with an indication of those papers that are particularly important for any given topic. The bracketed numbers refer to the Bibliography.

(a) *The third law of thermodynamics and specific heats*

When, in 1920, Simon began his work for the Dr. Phil., the Nernst heat theorem ('Mein Wärmesatz' as Nernst used to call it according to Simon's recollection) was fifteen years old. It had already proved its worth by enabling the prediction of chemical equilibria with the help of the postulate of vanishing entropy differences between condensed phases at 0 °K. The knowledge of the specific heats at low temperatures is important for such calculations; conversely, if the equilibrium is known, only through the extension of specific heat measurements to the lowest temperatures is it possible to prove the validity of the Nernst theorem. Although both these aspects were treated in Simon's thesis (1) it appears that from the earliest days Simon was mainly concerned with the validity of the theorem. The fact that the Nernst heat theorem is now regarded as the 3rd law of thermodynamics, equal in fundamental importance to the 1st and 2nd laws, is largely due to Simon's work and influence.

Simon's leading idea was that any apparent violation of the 3rd law was due either to incorrect extrapolation of the specific heats to 0 °K, or to the

fact that the system was not in internal equilibrium and hence thermodynamic arguments were not applicable to it. The 'Schottky' anomalies in specific heats were first predicted in 1921 (Schottky 1921, 1922) that is about the time when Simon finished his thesis. This prediction immediately roused Simon's interest in systems for which the transition into the ordered, non degenerate state occurs at low temperatures, corresponding to the small energy differences that are involved. He had already observed (1) a λ -type specific heat anomaly in ammonium chloride which he recognized as being of the 'co-operative' type (23). This anomaly was also investigated by X-ray studies (17) and by measurements of the infra-red absorption (32) and of the thermal expansion coefficient (44) on the chloride and on other ammonium salts.

The fact that the specific heats of certain cubic monatomic solids (e.g. grey tin, silicon) did not follow Debye curves led Simon to the hypothesis that this was due to superimposed Schottky anomalies (19); indeed, the measured specific heats could in many cases be well represented by the sum of a Schottky and a Debye function. For many years Simon had great faith in his 'quantum-jump' hypothesis, but gave it up willingly when the need for it disappeared with Blackman's refinement of the Debye theory. It must be admitted, however, that these hypothetical 'anomalies' gave Simon a fruitful stimulus and his 'anomaly-consciousness' was well known to his pupils and colleagues.

It was not till 1929 that the first real low temperature anomaly, of fundamental importance to the 3rd law, was discovered in solid hydrogen. Hydrogen had been something of a puzzle as far as the 3rd law was concerned: its chemical constant, as calculated from the vapour pressure, was found to be appreciably smaller than that obtained from chemical reactions, a behaviour that suggested a non-vanishing entropy for solid hydrogen at 0 °K. Simon had already earlier (19) suspected that a specific heat anomaly well below 10 °K might be responsible and the discovery of the ortho and para modifications gave firm theoretical basis for this hypothesis. The specific heat measurements carried out with Mendelssohn & Ruhemann (41, 48) indeed showed a large anomaly connected with the lifting of the threefold degeneracy of the orthohydrogen's rotational ground state and thus confirmed Simon's predictions. He returned to this same problem some 20 years later and with Hill & Ricketson (125, also Hill & Ricketson 1954) studied various ortho-para-systems down to about 0.8 °K.

The many anomalies found in paramagnetic salts are discussed below in (e), but mention must be made here of the specific heat measurements carried out with Parkinson & Spedding (106) on rare earth metals; the anomalies found in this work could all be explained, at least qualitatively by the Schottky hypothesis.

His own investigations and those of others convinced Simon that in the case of chemically homogeneous crystalline substances violations of the 3rd law could always be ascribed to unobserved specific heat anomalies occurring at

very low temperatures. This explanation however did not hold for amorphous solids and for solid solutions. Indeed experiments by Gibson & Giauque (1923) and by Simon & Lange (15) have shown that at 0 °K the entropy of amorphous glycerol—which might be regarded as a super-cooled liquid—is substantially greater than that of crystalline glycerol. Similarly Simon had found (14, p. 392) that the entropies of various glycerol-water mixtures differed from each other at 0 °K by the well-known mixing entropy term. In opposition to many other authorities Simon did not regard these cases as violations of the 3rd law. He argued that mixed crystals, solid solutions and glasses are not in internal thermodynamic equilibrium; 'their state is not determined solely by temperature, pressure and the number and kind of constituents' (128, p. 16), but is governed by the velocity of configurational changes which becomes very slow at the 'freezing-in' temperature. Such systems are not in their most probable state, hence they 'cannot be treated thermodynamically, as thermodynamics is concerned with the entropy of the most probable state only' (128, p. 16). Simon gave a striking proof of his contention by remarking that if a glass really did violate the 3rd law one should be able to reach absolute zero with it. This, however, is impossible because the state with the so-called 'zero-point entropy' cannot be brought reversibly into a neighbouring state. As a result of all these considerations Simon finally proposed the following, now generally accepted formulation of the 3rd law: 'The contribution to the entropy due to each factor within the system which is in internal equilibrium becomes zero at absolute zero' (81).

The graphite-diamond equilibrium, the classical example of the potential usefulness of the 3rd law was also reconsidered by Simon in collaboration with Berman (123) and their predictions have been confirmed by the successful diamond synthesis at the American General Electric Research Laboratory.

Simon wrote two review articles (14, 15) about the 3rd law and in his Guthrie lecture (128), delivered only about six months before his death, he gave a masterly historical account and an up-to-date appraisal of it. He admitted that it had now outlived its usefulness for the study of chemical gas reactions and believed that its principal function had become that of providing information about the state of condensed phases at absolute zero. It is interesting to note that as early as 1927 Simon suspected that the density-maximum of liquid helium at 2.2 °K might be connected with incipient 'liquid-degeneracy' in helium, that is a transition into an ordered liquid state (20). He was led to this supposition by the fact that the entropy difference between solid and liquid helium calculated from Keeson's melting curve, showed a rapid decrease below this temperature.

(b) Liquid and solid helium

Helium is the only substance which, under its vapour pressure remains liquid down to 0 °K. In one of his earliest papers (4) Simon suggested that this behaviour of helium could be explained by the relative importance of the zero-point energy compared with the thermal energy. The attractive forces,

unlike those in a normal liquid, cannot diminish the volume until they are compensated by the repulsive forces, because this would lead to a sharp increase in the zero-point energy. This is why the volume of liquid helium is abnormally large, the interatomic distance being $3.6 \text{ \AA}$ as compared with the gas kinetic diameter of $1.9 \text{ \AA}$.

Simon's first experiments on the thermal properties of liquid and solid helium were carried out in Breslau with Kaischew (57). The discussion of these results (58) showed that the solid-liquid transition in helium has many unusual features and it was mainly to study these in detail that after the war in collaboration with Swenson (102, see also Swenson 1951) he undertook further experiments on the melting pressure and on the volume changes during melting. These showed that the entropy difference between the liquid and solid phase diminishes very rapidly with decreasing temperature, in accordance with the 3rd law. This means that the heat of fusion becomes so small that the melting or the solidification of helium may be regarded as purely mechanical processes. When liquid helium is compressed at the equilibrium pressure it turns into solid with a decrease in volume; the external work appears as an increase of the internal energy of the solid over that of the liquid. In other words liquid helium at very low temperatures is unique in that its internal energy is smaller than that of the solid in equilibrium with it.

Another of Simon's contributions to the study of liquid helium was his discovery, with Rollin, of the helium II film (86, see also Rollin 1937). They found that the heat influx to vessels containing liquid helium appeared to be abnormally large below the λ -point. More precisely, when the vessel was being pumped the rate of evaporation was unaccountably high, and, when the vessel was shut off, the temperature rose rapidly. Having eliminated by a series of ingenious experiments various possible causes, such as a heat evolution occurring in the liquid helium itself, they concluded that the phenomenon must be caused by a film of liquid helium covering the connecting tube between the vessel and the warmer parts of the apparatus, and pointed to a possible connection between this phenomenon and Kamerlingh Onnes's (1922) puzzling experiments with communicating vessels. They have demonstrated, moreover, that the phenomenon must be due to a creeping film which evaporates on reaching the warmer parts of the tube and which, through recondensation, causes the abnormal warming-up rate of a closed vessel. The mobility of the liquid helium film was, of course, strikingly demonstrated in Daunt & Mendelssohn's (1938) classic experiments.

Simon's other work on liquid helium included the establishment of the helium vapour pressure scale below $1.7 \text{ }^\circ\text{K}$, done in collaboration with Bleaney (90), and experiments with Kurti on heat transport in liquid helium below $1 \text{ }^\circ\text{K}$ (82).

(c) *High pressures*

Simon's work on the properties of fluids at high pressures and low temperatures was chiefly governed by his idea of what he called 'model' experiments.

His point was that if one could show that the melting pressures of various substances obey the law of corresponding states it would be possible to predict from the properties of substances with low boiling points, i.e. small attractive forces, how other substances would behave under conditions that are difficult to realize in practice. He also hoped that this work would lead to a decision about the existence of a critical point for the fluid-solid transformation.

Simon's first series of high pressure experiments was done during the Berlin period, when, in collaboration with Edwards & Ruhemann, he determined the melting pressure curves for helium and several other low boiling fluids (34, 39, 42). A subsequent discussion showed that the law of corresponding states was indeed obeyed (74). That is, the results could be expressed by a unique semi-empirical 'reduced' melting pressure formula, in which the temperatures were expressed in terms of the normal melting point and the pressures in terms of the internal pressure, which is a measure of the attractive forces. These experiments went up to about 5000 atmospheres. In experiments carried out in Oxford after the war the melting curve of helium was followed up to 7300 atmospheres (101, see also Holland, Huggill & Jones 1951) corresponding to a melting point of 50 °K. In other words helium could be solidified at a temperature ten times as high as the liquid-gas critical point. This is a good illustration of helium being a 'model' substance: the corresponding conditions for nitrogen would be about 500 000 atmospheres and a temperature of about 1300 °K.

The validity of the melting point equation showed that there was no trend towards a solid-fluid critical point. A more sensitive way of proving this is to measure the entropy of melting which should tend to zero at the critical point. Detailed experiments on the thermodynamic properties of fluid and solid helium at pressures up to 3000 atmospheres, carried out with Dugdale (115) showed that the difference between the two phases as indicated by the entropy of melting becomes actually more pronounced at higher temperatures; there is thus so far no indication of a critical point. It is interesting to note that their results enabled Dugdale & Simon to confirm the validity of the Lindemann melting formula for *one single* substance over a substantial region of the melting curve.

The validity for low boiling fluids of his melting pressure equation encouraged Simon to apply it to a case of geophysical importance, namely the melting of iron (119). He calculated the melting pressure curve for iron up to 4×10^6 atm, corresponding to 4000 °K, and discussed these results in the light of recent ideas about the structure of the earth's core.

(d) *Thermal conductivity**

Simon's interest in heat conductivity was at first directed towards the effects of radiation damage. He realized that such a transport property would be very sensitive to the defects produced, and experiments made on quartz

* I am greatly indebted to Dr R. Berman for writing this section.

(103) and alumina (Berman, Foster & Rosenberg 1954) bear this out. This interest started a detailed study of heat conduction in dielectric crystals, a subject which had previously only received scant attention.

In a perfect dielectric crystal there are two processes which limit the phonon mean free paths and are thus responsible for thermal resistance (*a*) the Umklapp type of interaction between phonons, first proposed by Peierls, and (*b*) the scattering of phonons at the crystal boundaries, first studied in Leiden before the war. Peierls had predicted that the decrease in frequency of Umklapp processes at low temperatures would result in an almost exponential decrease in resistance with decreasing temperature. The new measurements on quartz, alumina and solid helium showed that this indeed occurred (107).

At the lowest temperatures the phonon mean free paths are only limited by scattering at the crystal boundaries, and the thermal conductivity becomes size dependent. This effect was also examined in detail, and the influence of specular reflexion of phonons at the boundaries was studied (118, also Berman, Foster & Ziman 1955).

More recently, it was realized that many departures from the ideal form of conductivity variation for otherwise perfect crystals, could be attributed to the irregularities represented by the random arrangement of isotopic masses. Work on this aspect of heat conduction started shortly before Simon was taken ill and the first paper from the Clarendon Laboratory (Berman, Foster & Ziman 1956) appeared a week after his death (see also Berman 1958).

(*e*) *Magnetic cooling*

When the method of cooling by isentropic demagnetization of paramagnetic salts was first proposed by Giaque (1927) and by Debye (1926) it was apparently not realized or at least it was not explicitly stated that here was a flagrant violation of the 3rd law. The magnetic entropy reduction possible in, say gadolinium sulphate, was so much larger than the lattice entropy at even 4 °K that there appeared to be no lower limit to the temperature that could be reached. It was clear to Simon that this state of affairs could not persist to arbitrarily low temperatures and he predicted a specific heat anomaly connected with the establishment of order with respect to spin orientations. The specific heat measurements carried out with Kurti (52, also Kurti 1933) indeed showed such an anomaly; they permitted the prediction of the temperatures that could be attained and they were the first example of the now common CT^2/R method for determining small energy splittings.

When in 1933 the Berkeley & Leiden demagnetization experiments became known (the results of Giaque & McDougall 1933 incidentally agreed with Simon's predictions) some of Simon's colleagues thought that he had been left behind in the race. As far as Simon was concerned there was never any question of a race, since he would have regarded it as contrary to scientific etiquette to try and compete with Giaque who was engaged in putting into practice his fundamentally new idea. This scrupulous observance

of a code of behaviour, to which he expected that others should conform, characterized Simon's attitude towards the competitive tendencies of modern scientific life.

It was soon after his arrival in Oxford, early in 1934, that Simon began his experiments with the magnetic cooling method. His work between 1934 and 1939, most of which he did with Kurti, followed three main lines. First of all there was the problem of establishing the thermodynamic scale in this new temperature range. Simon incidentally had tackled a similar problem some ten years earlier when, in experiments with Lange (3; for a brief description of this experiment see also Kurti 1958), he determined the thermodynamic scale between 10 and 14 °K directly from the 2nd law. The difficulty of introducing heat uniformly at temperatures below 0.1 °K where heat conductivities are notoriously poor, was overcome by the ingenious method of heating by γ -irradiation (66, 114). The first results on iron ammonium alum were published in 1935 (69) and subsequent measurements carried out in collaboration with Kurti & Lainé (75, see also Cooke 1949) with the large Bellevue magnet established the thermodynamic temperature scale down to 0.02 °K.

The knowledge of the absolute temperatures in the new temperature range permitted the study of the thermal and magnetic properties of these paramagnetic salts as a function of temperature. The main interest was to get information about the transition of the spin system into the ordered state. The very first results (69), showing two humps in the specific heat curve, clearly indicated the two mechanisms (1) a Schottky-type anomaly ascribed to the splitting of the ground-state by electric fields, and (2) a λ -type anomaly, associated with co-operative spin-ordering. The analysis of these specific heat curves also permitted a determination of the splitting constants (see particularly Hebb & Purcell 1937). The experiments in Bellevue with Kurti, Lainé & Rollin (72, 75) resulted in the discovery of unexpected phenomena below the temperature of the co-operative anomaly, viz. hysteresis, remanence and a positive temperature coefficient of the susceptibility, and these effects were further studied in collaboration with C. F. Squire (see Kurti 1951).

From the very beginning Simon regarded the study of other substances below 1 °K as one of the most important functions of the magnetic cooling method. It is significant that his first paper on adiabatic demagnetization (60) announced that cadmium became a superconductor at 0.6 °K. The comparative study of various paramagnetic salts (63), the development of techniques for cooling other substances into the new temperature range (70) were closely related to this interest. Besides the search for new superconductors (67) mention should be made of experiments on thermal relaxation and thermal conductivities (70) and on some properties of liquid helium (82).

The experiments on the establishment of the absolute scale below 1 °K and on the properties of paramagnetic salts both produced notable results,

but Simon will probably be best remembered (as regards magnetic cooling) for his lead in opening up this new temperature range for a wide variety of physical experiments. The influence of his fertile imagination and of his superb experimental ingenuity will be felt for a long time.

(f) Nuclear cooling and nuclear orientation

Simon has shown in the very first detailed paper on the magnetic method (63) that the cooling effect is, in the last resort, limited by the interaction energies. Hence if instead of an ordinary paramagnetic salt one used a substance whose paramagnetism is due to nuclear spins, one would reach much lower temperatures since the interaction energies are much smaller for nuclear magnetic moments than for electronic moments. A similar suggestion was made independently by Gorter (1934). In 1938 Simon discussed in great detail the whole question of nuclear cooling (88) and, in particular, pointed out the great experimental difficulties, such as the need of magnetic fields of 50 to 100 kOe, and of starting temperatures of about 0.01 °K. Simon's interest in nuclear cooling was not aimed at breaking low temperature records. Rather he expected that, just as ordinary magnetic cooling had given information about the nature and magnitude of interactions in paramagnetic solids, so nuclear demagnetization would lead to the same sort of information for systems of nuclear spins.

Nuclear cooling, or rather its essential prerequisite, nuclear polarization harboured another interesting possibility. It was in 1937 during one of Simon's visits to Bellevue that H. Halban, with whom Simon was discussing his plans, proposed various nuclear reaction experiments with polarized nuclei. Thus started a fruitful co-operation with Halban, who came to Oxford after the war, and led the team of nuclear physicists engaged on nuclear orientation work at the Clarendon Laboratory. Simon himself took only little active part in the radioactive decay studies with oriented nuclei that were carried out at Oxford. But his interest provided a constant stimulus and the first successful nuclear orientation experiment ever to be performed (Daniels, Grace & Robinson 1951) owed much to Simon's driving spirit and to his initiating the whole project.

Simon's main interest was in nuclear cooling and after lengthy preliminary work the first experiments were carried out in the early summer of 1956 in collaboration with Kurti, Robinson & Spohr (127). There was thus an interval of 22 years between the proposal of this method and its final realization. Simon was clearly elated by this success; but he regarded this experiment only as a preliminary phase, as a proof of the feasibility of the method, and he knew that a lot of work will have to be done before nuclear cooling could become more than the mere attainment of 10 or 20 micro-degrees absolute. It is significant that of his last two scientific papers, one, the Guthrie lecture, was in the nature of a broad retrospect of a life's work, while the other, on nuclear cooling, was like a first glimpse, achieved after years of laborious preparation, of a new field of knowledge.

(g) *Cryogenics*

The development of cryogenics during the last 50 years may be roughly divided into three phases. The first one was the period of a few, large, highly specialized low temperature laboratories. During the next phase small-scale liquefaction methods brought low temperature physics within the reach of non-specialized laboratories. Finally in the present phase, thanks to the commercially produced Collins liquefier, liquid helium has become an easily obtainable commodity. It was the second phase, which owed its existence largely to the influence of Simon and his school.

Simon developed the 'desorption' method for bridging the gap between hydrogen and helium temperatures during his Berlin period (18, 24, see also Mendelssohn 1931). In this method helium is adsorbed by charcoal at the temperature of liquid or solid hydrogen. When, after thermal insulation, the gas is pumped off, the temperature of the charcoal-helium system drops rapidly well below the critical temperature of helium. The desorption liquefier was soon replaced first by the miniature Linde liquefier developed by Ruhemann (1930) while he was working in Simon's laboratory and then by Simon's expansion method (51, 54) which is somewhat analogous to Cailletet's method for air liquefaction. But while Cailletet's experiments gave only a quickly vanishing mist of liquid air the method applied to helium is very effective for two main reasons. Firstly, at low temperatures the heat capacity of the vessel is negligible compared with that of the gas it contains. Secondly, at 12 °K a pressure of 40 atm is sufficient to give helium the same density as that of the liquid at the boiling point, while for air at room temperature a pressure of 3000 atm would be necessary. When helium at 12 °K is adiabatically expanded from 150 to 1 atm there is only a two-and-a-half-fold volume increase and 80 per cent of the gas liquefies; because of this relatively small volume change one can dispense with a moving piston and the expansion is done through a valve at room temperature.

Although the expansion method could also be used for the liquefaction of sizeable quantities of helium (91, see also Croft 1952) it was through the simplicity and cheapness of its small-scale application that it had such an effect on the growth of low-temperature physics at the Clarendon Laboratory and elsewhere. An important consequence of the use of these small liquefiers was that, since they were combined with the actual measuring apparatus, the Clarendon low-temperature physicists had direct experience with gas liquefaction and in many cases with the construction of liquefiers. Simon set great store by this arrangement, since he regarded skill in cryogenic techniques as an essential part of the low-temperature physicist's qualifications. He felt that a practical knowledge of gas liquefaction gave an excellent grounding in the thermodynamical basis of low temperature physics, and because of this he perhaps regretted in his heart that with the development of new facilities actual cryogenics came to assume a minor place in low temperature research.

Simon's scientific career roughly coincided with the period during which

low-temperature physics grew into an important, varied and unified discipline. The survey of Simon's researches shows that there was no topic of cryophysics that did not engage his interest at one time or another. Underlying his whole work was a profound appreciation and vivid understanding of thermodynamics which to him was a living subject drawing strength continuously from the interpretation of new phenomena. It was the breadth of his approach just as much as the importance of some of his particular contributions that made his influence so widely felt and which made him the outstanding low temperature physicist of his generation.

2. Atomic energy

It was in the early part of 1940 that Simon first heard about the possibility of using uranium-235 as a nuclear explosive. The problem of separating the uranium isotopes, which Peierls had already considered in general terms, interested Simon and he immediately began to work on it, first unofficially, then, by the end of the summer, with official sanction. The choice of the first members of his team was governed by the fact that everybody at the Clarendon, except aliens and ex-aliens, was already engaged on war work. So it came about that H. G. Kuhn, the spectroscopist, who came to Oxford from Germany at the same time as Simon, H. S. Arms, an American Rhodes Scholar, one of Simon's D.Phil. students, and N. Kurti were the first to join Simon in his work on the separation of the uranium isotopes by gaseous diffusion. To get quick results the early experimental work was rather improvised. Thus, although it would be an exaggeration to say, as has been done in some lighthearted reminiscences, that the first separation experiments at the Clarendon were done on soda-water with the help of Mrs Simon's kitchen strainer, the truth is not far off. In order to determine the separating efficiency of a diffusion membrane as a function of the mean free path, a fine mesh copper gauze was rolled down until the linear hole dimensions were about 10μ and for ease of analysis, a mixture of carbon dioxide and water vapour was used as test gas. Simultaneously with these experiments design studies for the arrangement of diffusion membranes in a plant and for suitable blowers were also carried out, and in December 1940 Simon prepared a report which contained the first realistic and detailed proposal for a sizeable uranium isotope separation plant. It speaks well for Simon's understanding of the engineering aspects that, in spite of the dearth of data, his estimates for capital cost, running costs, size, etc. turned out to be correct within a factor of two or three.

When it became official policy to give some priority to the Tube Alloys Project, as the British atomic bomb project came to be known, Simon's team at the Clarendon Laboratory grew and the work expanded. The physical properties of uranium hexafluoride were measured, the characteristics of various types of diffusion membranes were studied and methods for testing membranes were devised. Other possible separation processes, in particular Dirac's jet-method, were also investigated experimentally. There was a

particularly close collaboration between Simon's team and the team at I.C.I. Metals, Birmingham, which, under S. S. Smith, was responsible for the development of diffusion membranes. It was a cheerful and spirited co-operation from which Simon retained a warm affection for Smith. He also appreciated the value of Smith's vital contributions to the Atomic Energy Project both during and after the war which, he felt, did not receive the recognition they deserved.

Simon rapidly became a recognized authority on the diffusion separation method and was one of its most persistent champions. He believed that ultimately there would be a need for a diffusion separation plant in Britain and therefore when in 1944 most of the Tube Alloys work was transferred to the U.S.A. he was among those who favoured the continuation in this country of at least part of the work, so as to preserve effectively years of accumulated know-how and familiarity with the project. Thus work on membrane research in Oxford did not stop until 1947 while the membrane development work in Birmingham has continued without interruption. Simon's only regret was that none of the key members of the impressive plant design team at I.C.I. Billingham, could be transferred to the new organization, formed in 1946, for large scale atomic energy developments.

Through his membership of various committees Simon was also concerned with many other aspects of atomic energy (e.g. deuterium separation, the properties of metallic uranium) and his stimulating views, many of them unorthodox and hotly disputed, played their part in Britain's atomic energy developments both during and after the war. But his influence was perhaps most decisive in the early days when his realistic appraisal of the problems and his sense of urgency were largely responsible for the spirit of determination that characterized the Tube Alloys Project.

3. Science, industry and public affairs

During and after the war his atomic energy work brought Simon into close touch with many engineers, industrialists, business men and politicians, and his profound interest in certain wider aspects of science was mainly due to this experience. Although he was concerned with a variety of topics, these all seemed to have in common one basic idea, namely an uncompromising dislike of waste in all its manifestations. That is why he deplored the Government's lack of an integrated fuel and power policy and why he castigated so outspokenly the waste of fuel in old fashioned grates and the waste of manpower through the lack of good technological education, or through the ineffective use of the intellectual ability of the country.

It was undoubtedly Simon's thermodynamical outlook and the accompanying dislike of irreversible entropy increases that prompted many of his campaigns against waste. Some quotations from his Friday Evening Discourse at the Royal Institution (130) are characteristic. 'This love of squandering seems to be very deeply seated in humanity. It is not only the

schoolboy who is enthusiastic about the noise and prolific production of steam and fumes of that most uneconomical machine, the locomotive'. Later he says: ' . . . there seems to be a proportionality between the irreversible increase in entropy of some act and the pleasure it creates and I have often been tempted to use this for a definition of a unit of pleasure.'

The wasteful use of fuel for heating, especially in open fires, those 'contraptions to heat the stars and soil the earth' took pride of place among Simon's targets. He spoke with feeling when he said that 'the output of 100 thousand miners, working under uncongenial conditions, often risking their lives, is squandered unnecessarily, disfiguring the country' (139), and he fully agreed with Rumford, in Simon's view one of the first modern technologists, who said that open grates 'instead of making the room equally warm, do little more than increase the violence of those streams of cold air which come whistling in through every crevice of the doors and windows' (see 136). He hoped with Rumford that 'at some future period houses in England will become as celebrated for warmth and comfort as they are now for neatness' and in the late 1940's when coal was desperately short he proposed that the Government should offer, free of charge, simple stoves to fit into existing fireplaces. He believed that even with a moderate, 50 per cent response to this offer, the country could save 10 million tons of coal a year for a capital outlay of £50 million (131, pp. 51, 80, 85).

Besides castigating the wanton squandering of our fuel resources he was also trying to impress on people the wastefulness of using free energy to produce heat; hence his campaign against the use of electricity for heating on a large scale and his championship of the fuel-cell. He found, however, that 'to mention the fuel-cell, frequently means being suspected of not having heard of the second law of thermodynamics; if one further ventures to point out that there exist systems in which the output of free energy is higher than the change in total energy (i.e. the heat evolution), then one's knowledge of even the first law becomes suspect' (113). Simon recalled the embarrassed silence that greeted such a remark of his at a meeting attended chiefly by engineers.

Simon's campaign for fuel economy was largely prompted by the coal shortage during the decade after the war. He was also perturbed by the many over-optimistic statements made at the time about how nuclear power would shortly end all such troubles, since he felt that this would make people forget about the immediate problem. He never denied the great potentialities of nuclear power and he respected reasoned optimism since, as he said, 'we have, fortunately, an enthusiastic and most able team in our Atomic Energy Authority and if they were not optimistic they probably would not be the right people for the job' (142, p. 7). But he himself, when making predictions, erred perhaps on the cautious side and some people regarded his views as timid and unimaginative. Time will show who was right; it seems that the latest forecasts are not vastly different from Simon's.

It is important to realize that even if one admits the most optimistic

predictions, Simon's main thesis, that 'there seems no escape from the conclusion that nuclear energy cannot give us relief in our short term troubles, and coal will have to be our mainstay for a long time to come' (138) remains valid. A similar view has been expressed recently by Sir Christopher Hinton (1958) who, referring to the likelihood of a widespread use of nuclear electricity by the 1980's says: 'It should not be imagined that such development will lead to a reduction in the overall demand for coal.'

Simon, in common with many others, was worried about the shortage of scientists and engineers and he referred to this problem in many of his articles and lectures. He was concerned both about the total numbers and about the insufficient attention that was being given to higher technological education. With Cherwell he was in the forefront of the unsuccessful campaign for the establishment of an Institute of Technology modelled on the great Continental or American Institutions (141, 146, 148). He found our small output of scientists and engineers even more alarming when viewed against that of the U.S.A. and particularly of the U.S.S.R. Simon was among the first to recognize how shortsighted was the idea that in the atomic field 'the Russians can progress only by stealing secrets' (140). He pointed out, as early as 1951, that the picture of science in Russia being 'straight-jacketed' was a wrong one and that 'the majority of Russian scientists are more or less free to do as they like if only they pay lip-service to the official doctrines' (131, p. 92). In his broadcast talk 'The Soviet bid for technological leadership' (147) he voiced the opinion that the possibility of unfettered scientific thought, the high prestige of scientists, good science teaching at all levels and practically unlimited funds for research were the explanation of the spectacular growth of Russian achievements in science and technology. It is unlikely that the news of the Sputnik would have surprised Simon. In fact, many recent articles and talks emphasizing the growing disparity between our scientific and technological effort and that of Russia and demanding vigorous counter measures seem like echoes of Simon's warnings made several years earlier.

Simon believed that we were handicapped in our scientific and technological developments by the fact that 'the scientists and engineers—often condescendingly called "experts"—have too little say in the matters which should be primarily their concern. There are far too few of them on the boards of directors of industrial companies and hardly any—a most serious matter—in the higher Civil Service' (147). He thought this to be 'a consequence of the curious prejudice which has developed in Britain during the last hundred years that a real education is one restricted to the arts—contrary to the Greek ideal of a balanced education' (147). He was shocked by the number of 'educated people who seem to be proud of their ignorance of science' (139) and, referring to a subject particularly dear to him, he said: 'The first two laws of thermodynamics have become just as much part of our cultural inheritance as history and the classical languages; the impossibility of perpetual motion machines of the first and second kind

should be as familiar to the schoolboy as, say, the history of the Napoleonic wars' (113).

The Bibliography gives a selection of Simon's writings on these general topics. Many of the articles he wrote as scientific correspondent of the *Financial Times* between 1948 and 1951 were published in a little book, *The neglect of science* (131). Simon propounded his ideas relentlessly and almost with a crusading fervour, and was anxious that his views should reach as wide a public as possible. It is true that many people disagreed with him and that his more concrete suggestions were never put into practice. Nevertheless through the public discussions stimulated by his ideas and through his personal influence he played a part in shaping present-day thinking about problems of fuel and power, and about the place of science and technology in the life of the nation.

III. PERSONALITY AND INFLUENCE

The foregoing summary gives an idea of Simon's achievements as a scientist. But his greatness is due in equal measure to his human qualities and to his ability to stimulate others.

He spent practically all his life in universities, yet one of the main vehicles for communicating ideas, lecturing, was not his strength. He did not like lecturing, or indeed, any public speaking and when he was lecturing one often noticed a feeling of strain and tenseness unlike his usual relaxed and carefree manner.* He was, especially in the last years of his life, often asked to speak at banquets and similar functions and he viewed these invitations with very mixed feelings; he was too courteous to refuse lightly but having to speak was an ordeal for him. Besides he liked good food and wine and he would complain that, with an after-dinner speech hanging over his head, he could not enjoy his meal.

He could, and very often did, give superb lectures and addresses; for example, his two Friday Evening Discourses at the Royal Institution (65, 130), his Earl Grey Memorial Lecture (139) and his Guthrie Lecture (128) are masterpieces of clear and spirited exposition. But to achieve these results he needed careful and laborious preparation which he found a burden. It was in personal talk and discussion and in informal lectures that his breadth of knowledge and vivid understanding were most evident. He had an uncanny ability for recognizing the connexion between remote facts and for placing small details into the general picture. Many research students and co-workers have seen their apparently humdrum results acquire importance under Simon's scrutiny. It was his insistence on never losing sight of the wider implication, his refusal to get bogged down in detail (he disdained accuracy for accuracy's sake in physical measurements) that made work with him such a rewarding experience.

* The portrait illustrating this memoir, which was taken by Dr J. Wilks in 1955, shows Simon in such a characteristically happy and relaxed mood.

Sometimes when a particular topic caught his interest he would volunteer to give a series of informal talks and do so with apparent enjoyment. I recall his lectures given in Oxford in 1954 on some selected topics in thermodynamics (the third law, the meaning of temperature, etc.) when his enthusiasm was such that one had the feeling that new ideas were being born while he was talking.

Simon's influence as a teacher and founder of a great low-temperature school becomes evident in the large number of his one-time students and co-workers who have made their names in the world of science. He always took a fatherly pride in their achievements and from the time they joined his laboratory he became interested in their personal circumstances as well as their scientific work. He always met them socially as well as in the laboratory and the hospitality of 10 Belbroughton Road, the Simons' home ever since they first arrived in Oxford, was well known.

It may be said that the Clarendon low-temperature physicists, scattered though they were over many countries, formed something like a large family with Simon as its head. He continually corresponded with them, keeping alive their interest in each other and in the work that each was doing and on his many travels he always looked them up. Many of his secretaries married his pupils or co-workers so that they too became members of his 'family' which gave him great pleasure.

Simon's laboratory was a happy place and never dull, largely because his vitality and his acute interest in everything had such a stimulating effect. If some friction occurred—and no large department can avoid that—Simon would spend hours if necessary in patient talks to sort out the difficulties and with his sense of humour could often change an emotionally heated argument into an amicable discussion.

Simon was well known for his ability to clarify issues or to solve controversies by a single apt remark. If the speaker at a seminar presented his subject in an unnecessarily involved fashion Simon would ask a few simple questions that would bring out the fundamental points. At committee meetings his interventions were usually brief and to the point. On one occasion committee members were asked by the chairman, who was also in charge of the project, to agree that a certain machine be run at a power which was 10 per cent lower than the design value. Simon objected, arguing that 'design value' should mean what it said. Thereupon the chairman remarked: 'Professor Simon, don't you see that we are not talking about science, but about engineering which is an art.' Simon was persistent: 'What would happen if the machine were run at full power?' 'It might get too hot.' 'But, Mr Chairman', came Simon's rejoinder, 'Can't artists use thermometers?'

Although he was quick in repartee and fluent in expression, his constructions and phraseology were often quaint and he never lost his slight accent. Nor was he quite sure, especially in the early years of his stay, about some of the customs and traditions of this country. Thus, one day when

D. A. Jackson, the noted spectroscopist, came into the laboratory after a day's hunting, Simon asked how many foxes he had shot that day! He was, to the end of his life, conscious of these shortcomings and was perhaps unduly diffident about his English to the extent that he usually had his articles, often even his letters, vetted for errors of grammar or usage. But he did not take this very seriously and used to refer to himself jokingly as the 'Vice-president of the Broken-English Speaking Union', having ceded the Presidency, by mutual consent, to his good friend and colleague, the late Fritz London. This same gentle self-mockery, such an integral part of Simon's character, appears again in the last sentence of his review of Keesom's *Helium* (129): 'The book which is written in English contains a number of slight eccentricities of style and grammar about which, however, this particular reviewer has perhaps no right to complain.'

On reading Simon's articles one is struck by the freshness of his expressions and by his vivid metaphors. The opening of his Earl Grey Lecture (139) is a good example: 'If you saw a man open his pay-packet and throw half of it into the wind, you certainly would be rather surprised. And you would be even more surprised if you then heard that this man is short of money and always clamouring for more.' Here, in two brief sentences he drives home the absurdity of the waste of our natural resources and sets the tone of the whole lecture.

The same liveliness characterized his conversation. There was nothing polished about it but his obvious sincerity made up for the lack of elegance; he liked the give and take of conversation though often when something suddenly occurred to him, his impatience to say it became apparent. Many will remember his mannerism of giving light, scarcely audible grunts as if he were trying unsuccessfully to hold back his words.

Equally characteristic was his little note-book: Simon had an unreliable memory which, however, he came to regard as a blessing in disguise since it forced him to keep this note-book to record anything he wished to recall, from trivialities to important news. It became his inseparable and reliable companion, so that even in casual conversation he would bring it out to jot down information received or to look up some recent piece of gossip, scientific or personal. It only failed him on those occasions when even he himself could not decipher his own neat but illegible handwriting.

Simon's interest in his pupils, already mentioned, was a particular manifestation of his deep-seated humanity. As Professor P. W. Bridgman (1957) has pointed out: 'Perhaps his most outstanding trait was his interest in people for their own sake.' Indeed, Simon loved company; he liked meeting people and learning their views. His quick assessment of character and personality gave him an understanding that was often put to good use for, to quote from another appreciation (Cooke 1957): 'He was kindness itself in attending to other people's problems and his help was quick and generous.' His kindness sprang from an innate goodness and showed itself in many different ways, from putting a candidate at his ease during a 'viva' (he

employed various endearing stratagems for this purpose) to helping friends and colleagues leave Germany and to setting them on their feet again.

His being so intimately concerned with the problems of German refugees, many of whom found hospitality at the Simon's house, influenced his whole attitude towards Germany. When the Nazis came into power he was grieved, though perhaps not surprised, to hear how many of his colleagues, and more generally how many university professors had thrown in their lot with the Nazi regime, and this he could neither forget nor forgive. For those German colleagues who had the courage not to identify themselves with the regime and who retained their integrity his attachment became all the stronger and was tinged with warm admiration. After the war he carefully followed developments in German academic circles and came to view the future with grave misgivings. He saw that many of those who, thanks to political opportunism or conviction had flourished during the Nazi regime were still in important positions and were using their influence to the detriment of their anti-Nazi colleagues. He recognized the same tendencies in politics and as a result became convinced that the spirit that made the growth of Nazism possible was still alive. He made no secret of these views and he strongly opposed those who tended to forget and forgive the record of Nazi Germany as something that could not happen again.

Simon was a great traveller and he and Lady Simon regularly spent their holidays touring round Europe or resting somewhere in the Alps. Simon's accounts of his travels, his descriptions of landscape, towns and buildings were remarkably vivid and accurate. This was perhaps because he was an ardent and accomplished photographer and the scenes, as he photographed them, became engraved on his memory.

Simon regarded travel for visits to laboratories, for attendance at conferences and for personal discussion with colleagues as an important part of the scientist's life. He was indefatigable in getting funds to enable even junior research workers to go to meetings, and his money-raising technique ranged widely from light-hearted coaxing to impassioned appeals. His efforts to get financial support for his department were even more impressive and without his continuous solicitude for the finances of his laboratory and of those who worked in it, the Low Temperature Department of the Clarendon would not have flourished as it did.

In Oxford Simon was best known for his part in making the Clarendon Laboratory into a leading physics department. This has been recognized in the recent renaming of the wings of the Clarendon Laboratory, which are now known as Lindemann Building, Simon Building and Townsend Building. Although he took no active part in university administration his champion-ship of the cause of science in Oxford was effective. He loved college life and it was through common room conversations that his views, many of which were highly controversial, became well known. He enjoyed his frequent evenings in common room which he regarded as an essential feature of Oxford and one worthy of imitation. He once remarked in an after-dinner speech at an

American Faculty Club that high tables at Oxford or Cambridge were among the few places where a young lecturer and a Cabinet Minister could meet and talk to each other as equals; 'a useful experience for both of them', he added.

Simon's personality and demeanour underwent very little change in the course of years. When I first met him he was still relatively young, in his middle thirties, yet he was a fatherly sort of person who inspired respect and affection. But there was nothing pompous about him, and until the very end of his life he retained the impish streak in his character that endeared him to all who knew him. His sense of humour was, next to his goodness, perhaps his most important characteristic. He naturally disliked pomposity and any affectation of seriousness or pathos, and was even ready to relieve a genuinely serious occasion with some lighthearted remark. He liked to joke freely and often irreverently not only with his equals but also with his juniors and he could give way to boyish and infectious laughter which brought tears to his eyes. He often teased people but there was never any malice in his mockery. To quote a felicitous sentence from the obituary in *Nature* (Jones 1956): 'He was mischievous, mercurial, generous and warm-hearted, always approachable, impossible to offend.' Indeed, he enjoyed being the object of a good-natured leg-pull and could laugh heartily at his own foibles. His dislike of extreme temperatures as far as his personal comfort was concerned (his researches span seven temperature decades!) was proverbial and it was not surprising that at a Clarendon Christmas Party one of the posters, entitled 'Sir Francis' Thermodynamic Scale' should have depicted a large thermometer with three fixed points marked on it: 'Cap point' at 70 °F, 'Muffler point' at 68 °F, and 'Going home point' at 65 °F. Simon chuckled when he saw the poster and kept it as a memento.

The perfect blending of lovable subjective qualities with the disciplined objectivity of the physicist gave Simon's personality its cachet. His work, which brought him happiness and satisfaction, was imbued with his rich humanity and, conversely, his scientific outlook dominated his whole life. It was his avowed aim to impress on people that the pursuit of science was a rewarding and exciting adventure. He proved his point by the example of his own life and work.

Although I have known Simon and enjoyed his friendship for nearly thirty years, I could not have written this memoir without the help I have received from Lady Simon and from Simon's sister, Mrs L. Frank, to whom I wish to express my warmest thanks.

N. KURTI

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Carl Störmer

FREDRIK CARL MÜLERTZ STÖRMER

1874-1957

1. INTRODUCTION

THIS biography records the life of CARL STÖRMER, a son of Norway who added many important scientific achievements to his country's laurels. Born on 3 September 1874 at Skien, Norway, he died at Blindern on 13 August 1957, nearly 83 years old. I have no record of his ancestry or family background, but material concerning his childhood is given in an interview published in 1925 in a journal (*Varden*) not available to me.

In March 1956 he sent me a brief list of 'biographical notices' about himself—perhaps realizing that I might be invited to write such a notice as this. It contained several interesting particulars herein incorporated, which otherwise would not have come to my notice. In this typed list the *o* in his surname is written *ö*, not *ø*.

He was the only child of Georg Ludvig Störmer (an apothecary or dispensing chemist, first in Skien, later in Oslo), and Henriette Störmer, née Mülertz. From his childhood he showed a deep interest in the natural sciences, astronomy, physics, chemistry, meteorology, geology and in particular botany. At the age of about sixteen his interest turned exclusively to pure mathematics.

He was a student at the national university at Christiania (now Oslo) from 1892 to 1897; in 1898 he became a *candidatus realium* (roughly on the academic level of a Ph.D., but not carrying the title of Doctor); he gained a university stipendium (a five year research fellowship) in 1899. He continued his mathematical studies at the Sorbonne in Paris from 1898-1900, and at Göttingen in 1902. At the Sorbonne he studied under Picard, Poincaré, Painlevé, Jordan, Darboux and Goursat. He gained a good knowledge of the French and German languages.

In 1900 he married Ada Clauson, who survives him. They had five children. Precocious as a mathematician, he published original papers on the subject from his earliest student years—one in 1892 (aged 18) on the summation of trigonometric series, three more in 1895, and a further three in 1896. In each of these last two years, also, he published short botanical notes—tokens of an interest in living Nature that persisted throughout his life. I recall that when he visited me in London in 1930 to give a short series of lectures at the Imperial College, he brought with him a botanist's metal collecting case, hoping to find flowers in England that were new to him; the month, unfortunately, was unpromising—February—and the only

flowers readily available were those of groundsel, of which, he averred, Norway had an ample supply. In 1944 Knut Thalberg produced a list of Störmer's papers up to 1943, partly based on Störmer's own records; this list includes further botanical papers in 1925 and 1943.

Störmer continued to produce papers on pure mathematics: one in 1897, another in 1898, two in 1899, two in 1900, one in 1901, and in 1902 as many as five. Their subjects were series, theory of numbers and theory of functions. In 1902 he also helped to edit (with Holst and Sylow) two memorial volumes to celebrate the centenary of the birth of Norway's short-lived mathematical genius Niels Henrik Abel—a Festschrift in Norwegian (374 pages 4°) with a translation in French (460 pages 4°); he also prepared a study, published in Norwegian (62 pages) and in French, of documents left by Abel; and with Guldberg he edited a posthumous paper by Sophus Lie on integral invariants and differential equations. In these editions, and in general throughout his life, he gave his name as Carl Störmer, omitting his first and third names.

In 1903, at the age of 29, he was appointed professor of pure mathematics at Oslo—a position he occupied for 43 years, retiring in 1946 at the age of 72. By enquiry of Professor S. Rosseland, I learn that there was one other candidate, Guldberg. The expert committee for the appointment consisted of J. Petersen and H. G. Zeuthen (Copenhagen), F. Klein (Göttingen), E. Picard (Paris) and L. Sylow (Christiania). Zeuthen, Picard and Sylow preferred Störmer, the other two were neutral. 'After some haggling in the Faculty', the candidates were placed in the order (1) Störmer, (2) Guldberg; and the government acted on this basis. I have found reason to believe that present-day pure mathematicians would not dissent from this view of the relative merits of the two candidates, as pure mathematicians. Certainly Norway must be congratulated on providing Störmer, by this appointment, with a good economic foundation for his life-long researches, that brought fame to him and his country. He gave a grand survey of his major works in his book *The polar aurora* published in 1955.

These, however, proved to be very different from what was to be expected from Störmer's work up to that time. Pure mathematics was not left out of his thoughts. In 1903 he published three more mathematical papers, and a note on a letter from Abel to Kūlp. Further brief notes on pure mathematics appeared in 1908, 1933, 1937, 1938, 1940, 1942, 1943, and 1944. He lectured on many topics in pure mathematics, and produced stencilled notes on some of his courses, in 1924, 1932, 1933, and 1942. He occasionally reviewed mathematical books, and he wrote many biographical notes on mathematicians, such as Abel, Lie, Darboux, Holst, Sylow, Thue, Jordan, Mittag-Leffler, and Ramanujan.

Professor L. J. Mordell informs me that Störmer was expert in number theory and allied subjects. The mathematical problems he dealt with are of considerable interest and importance. Their solution was not easy, and his methods were elegant. In 1895 he showed that there are only four non-

trivial solutions of the equation $m \tan^{-1}(1/x) + n \tan^{-1}(1/y) = k\pi/4$, where m, n, k, x, y are integers. He returned to this subject in 1943 after his results had been extended by others to allied problems. This subject led him in 1897 to several problems connected with the Pellian equation $x^2 - Dy^2 = \pm 1$; he completely solved the problem of determining when every prime p dividing y also divides D . 'His result is very pretty, and there are many applications of it.' Two papers (1900, 1902) relate to Diophantine approximation, e.g. the question of approximating to an irrational number ξ by means of a rational fraction x/y . Thus if t is a given number, what can be said about a function $f(t)$ such that there is an integral solution in x, y of $|x/y - \xi| < f(t)$, with $0 < y < t$, or again when there is no solution for all t ? Störmer considered the second problem in some entirely new cases, e.g. when $\xi = \log \alpha / \log \beta$, where α, β are algebraic numbers, and again when ξ is the quotient of two elliptic integrals. He showed that we can take for $f(t)$, $e^{-\omega t}$ and $e^{-\omega t^2}$ respectively, where ω depends only on ξ . Results of this type have attracted much attention in recent years, and Störmer's results are well known.

2. TRAJECTORIES OF ELECTRIC CHARGES IN THE GEOMAGNETIC FIELD

In the year of Störmer's appointment to the chair at Oslo, his colleague Professor Kr. Birkeland showed him a series of beautiful experiments on the movements of cathode rays in magnetic fields, particularly in the field of a magnetized sphere or magnetic dipole. The event permanently diverted Störmer's research interest from pure mathematics to auroral problems. Birkeland in 1896 had proposed that auroras are caused by electrons shot from the sun, and deflected to high latitudes by the earth's magnetic field. Poincaré in 1896 had integrated the equations of motion of a charged particle in the field of a magnetic *pole*. Their work impelled Störmer to study the more complicated and realistic problem of the motion of a charged particle in the field of a magnetic point *dipole*. His first contribution on the subject formed his only paper published in 1904.

In 1905 he became a co-editor of *Acta Mathematica*; his only publication that year was a 31-page description of the scientific papers left by Sophus Lie. But the publication in 1906 of three short papers in the Paris *Comptes Rendus* on electron trajectories in the geomagnetic field, and on Villard's experiments on such orbits, gave a foretaste of the results of his active work on this difficult problem.

The first extensive exposition of these researches was given in 1907, in a series of papers (in French) in the *Archives des sciences physiques et naturelles de Genève*. He found first integrals of the equations of motion of a charged particle moving solely under the influence of the field of a stationary magnetic dipole. The speed is clearly constant. From the first integrals he was able to draw valuable and illuminating inferences concerning the nature of the trajectories. The complete solutions required numerical integration. Over many years he made or organized such calculations, finding a great variety

of orbits. His papers on this subject constitute a major part of his life's work; they number at least 48. Many are short, and in several cases he published brief accounts of the same work in different languages and journals. But many of the papers are very substantial in length and content.

They fall into a number of series. One (A) began with the Geneva papers of 1907 (numbering 137 pages in all); it continued in the same journal in 1911 (101 pages) and 1912 (55 pages); he also published a number of shorter notes there at different times. This series was entitled 'Sur les trajectoires des corpuscules électrisés dans l'espace sous l'action du magnétisme terrestre avec application aux aurores boréales'.

Another series of four papers (B) began in 1907, and was continued in 1913 and 1916 (two papers); the title was 'Sur un probleme relatif au mouvement des corpuscules électriques dans l'espace cosmique'. It was published in the *Skrifter* of the Oslo Academy, which in 1912 and 1916 also contained two papers (series B¹) entitled 'Quelques théorèmes généraux sur le mouvement d'un corpuscule électrique dans un champ magnétique'.

Overlapping with these series (B, B¹), a new series (C), which also appeared in the *Skrifter*, began in 1913, under the title 'Résultats des calculs numériques des trajectoires des corpuscules électriques dans le champ d'un aimant élémentaire'. This series consisted of eight extensive memoirs. Long intervals intervened between the first three (1913) and the next two (1936), and again between the appearance of these and the final three (1947, 1949 and 1950).

The first long gap in the succession of the trajectory papers extended from 1916 to 1930. In the meantime, E. Brüche had repeated and further developed Birkeland's experiments on beams of cathode rays projected into a magnetic dipole field; his admirable photographs of their form strikingly confirmed some of Störmer's theoretically calculated paths. This work has more recently been continued by W. H. Bennett, who gave to his excellent equipment for the purpose the name Störmerton.

Moreover Störmer became aware of the existence of certain short period pulsations of the geomagnetic field, which Eschenhagen was among the first to recognize as worldwide in extent; more recently they have been studied by Grenet, Kato, Kalashnikov, Troitskaya and others; the periods range from a few seconds to some minutes. He thought that these pulsations might be explained by the magnetic field of electrons moving in periodic orbits such as those whose existence had been revealed by some of his calculations. These developments inspired the production in 1930 of a paper in the *Zeitschrift für Astrophysik* entitled 'Periodische Elektronenbahnen im Felde eines Elementarmagneten und ihre Anwendung auf Brüches Modellversuche und auf Eschenhagens Elementarwellen des Erdmagnetismus'. This was followed in 1931 by a paper in *Terrestrial Magnetism*, entitled 'On pulsations of terrestrial magnetism and their possible explanation by periodic orbits of corpuscular rays'. The years 1931 and 1932 saw the publication in the *Zeitschrift für Astrophysik* of a series (D) of three papers on 'Ein Fundamental-

problem der Bewegung einer elektrisch geladenen Korpuskel im kosmischen Raume'.

Another development that closely concerned Störmer's work on the trajectories was the growth of interest in cosmic rays among many physicists. In 1931 he drew attention to the application of his theoretical papers to cosmic rays. In 1933 Rosseland, on his return from Boston, Mass., U.S.A., brought to Störmer's notice the recent work on cosmic ray paths by Lemaitre and Vallarta, performed on the differential analyzer at the Massachusetts Institute of Technology. This stimulated Störmer to publish in 1934 in the *Physical Review* a critical commentary on the paper by Lemaitre and Vallarta; rather too severely he noted some errors therein, but he did not realize the importance of their application of Liouville's theorem to these problems. In 1933 he began a new series (E) of six papers entitled 'On the trajectories of electric particles in the field of a magnetic dipole with applications to the theory of cosmic radiation'; the series was continued in 1934 (two papers) and in the years 1935, 1936, 1937. The first two appeared in the Oslo *Avhandlingar*, the remainder in *Astrophysica Norvegica*.

3. OUTLINE OF STÖRMER'S MAIN 'TRAJECTORY' RESULTS

The several long series of papers briefly recounted in § 2 almost all dealt with the trajectories of a particle of mass m , charge $-e$ (e. m. u.) and velocity $\mathbf{v}$ in the field $\mathbf{H}$ of a point magnetic dipole of moment $\mathbf{M}$. The vector equation of motion has the simple form

$$m\dot{\mathbf{v}} = -e\mathbf{v} \times \mathbf{H}, \quad (1)$$

where $\times$ is the symbol for the vector product operation on two vectors. As the force is perpendicular to the velocity, the speed v is constant. This is one 'first integral' of the equation.

Störmer simplified the equation by adopting C_{st} as the unit of length, where

$$C_{st} = (Me/mv)^{\frac{1}{2}}. \quad (2)$$

He took the origin at the dipole, and the z axis along the direction of $-\mathbf{M}$. He used both spherical and cylindrical polar co-ordinates, r, θ, ϕ and R, z, ϕ , so that

$$r^2 = R^2 + z^2. \quad (3)$$

Let an accent over a symbol denote differentiation with respect to the path length s along the trajectory, and let a dot denote partial differentiation with respect to R .

The equations of motion can be written

$$R'' = R(\phi')^2 + (r^2 - 3z^2)R\phi'/r^5, \quad (4)$$

$$z'' = 3zR^2\phi'/r^5, \quad (5)$$

and a third equation which is integrable, with the solution

$$\phi' = 2\gamma/R^2 + 1/r^3, \quad (6)$$

where γ denotes a constant of integration, that can have any positive or negative (or zero) value; (6) is another first integral of (1).

If θ is the angle between the tangent to the trajectory, and the meridian plane through the particle, so that $\sin\theta = R\phi'$, it follows from (6) that

$$\sin\theta = 2\gamma/R + R/r^3.$$

This equation gives the value of γ for the trajectory through any chosen point R, z for a given value of θ . But since

$$-1 \leq \sin\theta \leq 1,$$

the trajectories cannot go through the points in space where $2\gamma/R + R/r^3$ is greater than 1 or less than -1 . This condition defines boundaries between the regions that are *forbidden* to the trajectories and the regions in which trajectories are *allowed*. The boundaries are in all cases surfaces of revolution about the dipole axis. The meridian sections of these surfaces for $\gamma < -1$ have three branches; two are ovals which at the origin, where they meet, are pointed, the third is a curve extending to infinity at both ends. These curves divide space into two separated allowed regions.

Only if $-1 < \gamma < 0$ does the allowed space stretch without interruption from infinity to the origin (which is a singular point of the differential equations, of a very complicated kind). For *small positive* values of γ the meridian sections of the allowed regions have 'horns' projecting towards the dipole, above and below the plane $z = 0$.

From these equations z can be obtained in the form

$$z = f(R, C_1, C_2), \quad (7)$$

where C_1 and C_2 are two constants of integration, which arise in the solution of an equation of the form

$$\ddot{z} = q_0 + q_1\dot{z} + q_2(\dot{z})^2 + q_3(\dot{z})^3, \quad (8)$$

where the q 's are functions of R, z and γ . Using (7), it is possible by a quadrature to obtain s as a function of R ; finally ϕ is found by a further quadrature. Thus the paths are completely determined by this procedure.

The paths in the equatorial plane of the dipole, $z = 0$, can be treated more simply, and expressed in terms of elliptic functions. Störmer showed that their radius of curvature is equal to R^3 . The paths include some that come from infinity, and others that are periodic. They include a circle, an equilateral hyperbola, and the z axis. These are the only *algebraic* curves in the whole family of trajectories.

Störmer made another transformation of the equations by introducing new variables, eliminating the constant γ . The new variables R_1 , z_1 , r_1 and τ , which replace R , z , r and s , are defined thus if γ is positive:

$$R_1 = 2\gamma R; z_1 = 2\gamma z; r_1 = 2\gamma r; \tau = 8\gamma^3 s. \quad (9)$$

If γ is negative, a minus sign is inserted on the right of each equation in (9).

When γ is positive, let

$$U = -\left(\frac{1}{R_1} + \frac{R_1}{r_1^3}\right)^2 \quad (10)$$

The equations of motion can be written thus:

$$\frac{d^2 R_1}{d\tau^2} = \frac{1}{2} \frac{\partial U}{\partial R_1}, \quad \frac{d^2 z_1}{d\tau^2} = \frac{1}{2} \frac{\partial U}{\partial z_1}, \quad \left(\frac{dR_1}{d\tau}\right)^2 + \left(\frac{dz_1}{d\tau}\right)^2 = U + W_0^2, \quad (11)$$

where $W_0 = 1/4\gamma^2$.

The same set of equations applies when γ is negative, except that in this case

$$U = -\left(-\frac{1}{R_1} + \frac{R_1}{r_1^3}\right)^2. \quad (12)$$

When $\gamma = 0$, one obtains the same set of equations involving R, r, z, s instead of R_1 , etc., if

$$U = -R^2/r^6, \quad W_0 = 1. \quad (13)$$

Thus instead of studying the equations (4 to 6) for an infinite set of values of γ , only the set (11), in its three forms, needs consideration; this is a most important reduction, obtained by Störmer in 1913.

He made yet another transformation, which simplifies the numerical calculation of the trajectories. It proved useful in the machine calculations by Lemaitre and Vallarta in their cosmic ray studies. Let

$$R + iz = e^{u+iw}, \quad ds = e^{2u} d\sigma, \quad (14)$$

and let the dot over a symbol now denote $d/d\sigma$. Then the equations of motion may be transformed to

$$\ddot{u} = e^{2u} + 2\gamma e^{-u} + e^{-2u} \cos^2 w, \quad (15a)$$

$$\dot{w} = e^{-2u} \sin w \cos w - 4\gamma^2 \sin w / \cos^3 w, \quad (15b)$$

$$\dot{u}^2 + \dot{w}^2 = e^{2u} - 4\gamma e^{-u} - 4\gamma^2 / \cos^2 w - e^{-2u} \cos^2 w, \quad (15c)$$

$$\dot{\phi} = 2\gamma / \cos^2 w + e^{-u}. \quad (15d)$$

Here $R = 0$, $z = 0$ correspond to $u = -\infty$, $w = \pm(\frac{1}{2})\pi$; that is, the

singular point (the origin) is removed to infinity, as is desirable in the numerical work.

Defining P by

$$P = e^{2u} - 4\gamma e^{-u} - 4\gamma^2/\cos^2 w - e^{-2u} \cos^2 w, \quad (16)$$

the system of equations can be expressed thus:

$$\ddot{u} = (\frac{1}{2})\partial P/\partial u, \ddot{w} = (\frac{1}{2})\partial P/\partial w, \dot{u}^2 + \dot{w}^2 = P. \quad (17)$$

Similar equations may be obtained in variables u, w, σ similarly defined in terms of R_1, z_1, τ for the three cases γ positive, negative or zero, relative to slightly different forms of P .

The equation (15d) may be transformed by introducing a new variable ω given by

$$\tan \omega = dw/du,$$

that is, ω is the angle between the tangent to the integral curve and the axis of abscissa, if u is chosen as abscissa and w as ordinate. Then (15, a, b, c) become

$$\dot{u} = K \cos \omega, \dot{w} = K \sin \omega, \dot{\omega} = (\partial K/\partial w) \cos \omega - (\partial K/\partial u) \sin \omega,$$

where $K = \sqrt{P}$.

If u, w and ω are interpreted as Cartesian co-ordinates in space, these three equations give the direction of the tangent to the integral curve at each point. This yields a method of discussing the curves and the actual trajectories.

These transformations are set out in more detail in Chapter I of Part II of Störmer's book *The polar aurora*. In Chapter II he gives some main inferences drawn from the direct study of the differential equations.

The immense calculations by Störmer and his assistants, to explore the great variety of possible orbits, were extended by Lemaitre and Vallarta, with special reference to cosmic ray problems. Alfvén, using a perturbation method, obtained curves that in many cases approximate closely, in certain regions, to the true trajectories, and can be found with far less trouble of computation.

Störmer illustrated many of his results by wire models and by drawings. Close correspondence was found between his trajectories and the paths of narrow bundles of cathode rays in a dipole magnetic field, in the experiments by Birkeland, Brüche, Malmfors, Bennett and Brunberg. Störmer suggested that such experiments might indicate the trajectory shape, in some cases, more quickly than by numerical computation.

In all this work a fundamental parameter is the rigidity of the particle in relation to the magnetic field. The stiffness is defined as $H\rho$, where H denotes the magnetic intensity, and ρ the radius of curvature of the trajectory at the point:

$$H\rho = mv/(-e)$$

This is related to the Störmer length unit as follows:

$$C_{st} = (M/H\rho)^{\frac{1}{2}} \text{cm.}$$

4. STÖRMER'S APPLICATIONS OF HIS CALCULATIONS TO THE ACTUAL AURORA

Störmer considered that his mathematical and computational work on the particle trajectories gave promise of explaining many features of actual auroras. He considered alternative possibilities regarding the particles—they might be electrons or protons. For a range of the rigidity $H\rho$ from 100 to a million, the particle energy in electron volts ranges from 880 to 3×10^8 for electrons, and from 0.48 to 48 million for protons. The Störmer distance correspondingly decreases from 9 million to 90 000 km, the same for either kind of particle.

Störmer's theory of forbidden regions indicated very simply that a particle entering the earth's atmosphere, and coming from a distance greater than C_{st} , cannot approach the magnetic pole nearer than a limiting north polar angle θ . This he associated with the poleward limit of the auroral zone. 'Here we encounter the first disagreement between theory and observation. For cathode rays the auroral region is too near the magnetic axis, and even for protons the region of maximum frequency is further south than the theoretical one. For aurorae during violent magnetic storms, the difference is even greater, as the aurora in these cases may be observed overhead even in central Europe' (*Polar aurora*, p. 296).

In his second Geneva paper (published in two parts, in 1911 and 1912), Störmer 'tried to remove the difficulty of explaining the real radius of the aurora zone by admitting an action on the moving corpuscle of a stream of other corpuscles round the earth . . .' (*Polar aurora*, p. 340). Negative particles shot towards the earth from the sun, in the plane of the dipole equator, tend 'to bend round the magnetic globe on the evening and night side . . . and even to encircle it like a ring. On the morning and noon side, however, they are thrown away from the geomagnetic equatorial plane' (*ibid*, p. 341). Störmer investigated the magnetic field of such electron streams, treating them as a complete ring round the earth (thus making a mathematical approximation legitimate in such an exploratory study). The electric current flow is westward. He found that the magnetic field of such a ring current could draw the aurora zone nearer to the tropics; the same effect was experimentally demonstrated by Brüche in 1930.

This work is the origin of the 'Störmer ring current', still often referred to. Its dimensions, as given in his Geneva paper, located it beyond the moon's orbit. The magnetic field needed to move the aurora zone down to the latitudes sometimes reached, would profoundly modify the geomagnetic field over an extensive volume of space around the ring current, though causing no more than the observed magnetic storm changes near the earth's equator. The field near the ring would reverse the dipole field there. The ring was a mathematical idealization of electron streams mainly sweeping partly round the earth.

In *Polar aurora* Störmer did not reproduce his Geneva specifications of the ring current, and attached no decisive importance to his conclusions, though regarding them as pointing in the right direction (p. 345).

More recent speculations on the theory of magnetic storms and auroras, by Ferraro and myself and others, still contemplate the possibility of a ring current, but of a size and kind quite different from those above mentioned; the current is tentatively located within a few earth radii from the earth's centre, and there it only fractionally alters the dipole field. The existence of a ring current, inferred from purely geomagnetic evidence by Ad. Schmidt in 1916, still remains a speculation; no clear indication of its mode of formation has yet been given. Satellite observations may ere long confirm or refute the current conceptions on this subject.

Störmer tried to explain the limitation of the auroral zone on the side towards the pole by reason of (a) the origin of the particles in the sun, whose declination has a limited range, between $\pm 23\frac{1}{2}^\circ$ relative to the geographical axis, or (what is here more relevant) between $\pm 35^\circ$ relative to the geomagnetic axis; and (b) of the nocturnal appearance of the aurora, because trajectories that meet the earth on the side away from the sun correspond to γ less than about -0.5 . Störmer recognized (p. 298, *loc. cit.*) that also this explanation met with difficulties. 'It may be possible that' auroras seen in high geomagnetic latitudes 'can come from sources other than the sun', or that the simplifying assumptions on which his theory was based are too restrictive.

He showed that the particles issuing from the sun in a narrow cone can impinge on the night side of the earth within a narrow belt of geomagnetic latitude: he proposed this as the explanation of the frequent appearance of thin auroral arcs lying nearly in the direction magnetic east to west; he extended this explanation, with detailed numerical examples, to the case of folded auroral arcs.

In his 1912 Geneva paper (which contains a correction to his 1907 Geneva publications) he considered how the trajectories would be affected by taking into account the non-dipole terms in the geomagnetic potential.

5. CRITICISMS OF STÖRMER'S THEORETICAL WORK ON THE AURORA

One short chapter in Störmer's book *The polar aurora* is entitled 'What the mathematical theory can or cannot explain'. The meaning of *explain* as here used is perhaps ambiguous, but the chapter is modestly frank about what the theory cannot explain. It indicates clearly the simplifying hypotheses adopted at the outset of the theory. The most serious of these is the neglect of the action of the other particles on the single particle considered. Schuster criticized Birkeland's theory of aurora and magnetic storms on the ground that a stream of particles all having charges of the same sign would be dispersed by their mutual repulsion. Lindemann (later Lord Cherwell) proposed a neutral ionized stream as the cause of auroras and magnetic storms. This view is now generally adopted, and it has been shown by Ferraro and myself (1940), in an idealized problem, how low must be the density of a neutral ionized stream if it is valid to neglect, as in Störmer's

theory, the interactions of the particles; a stream of such low density could not produce the observed magnetic effects upon the earth.

To the writer it has long seemed that Störmer's theoretical work did not attain its main objective, and that auroral theory must be developed along radically different lines. Störmer himself seemed unmoved by such criticisms. His book *The polar aurora* is devoted almost wholly to his own work, and is a valuable unitary presentation thereof. He has, however, included a brief summary (p. 339) of an attempt by Vegard to meet Schuster's criticism, by supposing that the stream of solar particles, though electrically neutral, carries an electric current. An alternative attempt to meet Schuster's criticism has been given in recent years by Hulburt and Bennett, and criticized by Ferraro and by Vegard. In a late chapter of *The polar aurora* Störmer gave brief summaries of other theories of the aurora, with a few critical remarks.

The book has a good author's index and table of contents, but its subject index might, with advantage to its readers, have been much fuller.

6. STÖRMER'S THEORY AND COSMIC RAYS

If Störmer's theoretical work did not reach his objectives, and omitted vital factors in auroral causation, it is the more fortunate that it turned out to have an important field of usefulness, originally totally unexpected, in connexion with cosmic rays. As he truly said, 'The objections raised against the validity of the application of this theory to the aurora, are not as serious when the theory is applied to cosmic rays. In fact, since the energy of the cosmic corpuscles is so great and their concentration in space so small, the action of streams of such corpuscles on each other may be neglected' (*The polar aurora*, p. 375). As already indicated, Störmer made numerous valuable contributions, both critical and constructive, to this subject.

7. STÖRMER'S PHOTOGRAPHIC AURORAL STUDIES

Störmer's theoretical work on the aurora stimulated him to make observational studies, from 1909 onwards. This quite different part of his researches has earned for him an undying fame in the history of auroral science, quite independent of the theoretical work so far described. In *The polar aurora* he gave first place to the observed data about auroras, leaving the account of his theoretical work to Part II. He was an enthusiastic and indefatigable observer and organizer of observations. His last paper, that appeared in the year of his death, was an account of Norwegian auroral observations in the preceding year, 1956.

In his preface he remarked that in 1909 he 'found it necessary to obtain more facts about the aurora in order to compare theory and observation. A photographic method designed to determine, among other things, the height and position of aurora was therefore developed and successfully applied. The chief results obtained from the analysis of a vast number of parallactic photographs are discussed in this book.'

Though the aurora was first photographed in 1892, Störmer in 1909 began systematic experiments to assure easy and successful auroral photographs, as a preliminary to an auroral expedition he made to Bossekop in northern Norway in 1910. There he set up two stations for parallactic photography, to determine the height and location of the auroras that were so much more often visible at Bossekop than at Oslo. The base line between the two stations, $4\frac{1}{4}$ km, proved to be too small: the experience gained in this expedition was used in 1913 in a second expedition which he organized, also to Bossekop, this time using a base line of $27\frac{1}{2}$ km. About 2500 determinations of height and position of selected points of the aurora were obtained. In all he obtained more than 40 000 auroral photographs.

From 1911 up to his last years Störmer continued this parallactic photography in southern Norway. He amassed more than 9000 sets of pictures, from which he derived the heights and locations of more than 18 000 auroral points. His work involved telephonic links with his co-operating observers, at the other ends of his base lines; and the Norwegian telephone authorities gave him good facilities of this kind. He used the help of many assistants during this long-continued work, both for the actual photography, and for the work of measurement and calculation from the plates.

Apart from the nocturnal effort involved in getting the photographs, their reduction was a herculean labour, in which Störmer showed the same perseverance and tenacity as he manifested in his theoretical work. In measuring his early Bossekop plates he developed a method of projecting the negatives on a wall: the magnified image facilitated identification and measurement of the same auroral points on two or more plates. At first the reductions from the measurements were made by numerical calculation, but gradually graphical methods were adopted. In the evolution of appropriate methods, Störmer's own ideas were supplemented by those of Vegard and Krogness, Harang and Tønsberg, Herlofson and Egeberg, most of whom acted for a time as his assistants. These methods are rather fully described in chapter 5 of *The polar aurora*.

The results of this work were described in a long series of extensive memoirs, mainly in the *Geofysiske Publikasjoner* of the Norwegian Academy. Störmer was the first to establish beyond doubt the height of the aurora, as to which, despite some notable earlier attempts, there was great uncertainty when he began his work. He gave valuable statistical discussions, and diagrams that summed up the results of his observations; these (and similar researches by others) are described in chapters 6 to 8 of *The polar aurora*. His work stimulated others in different parts of the earth to observe and calculate auroral heights and positions—notably during the second International Polar Year. He actively encouraged and helpfully advised others who took up such work; for many decades he was the acknowledged leader among auroral observers. He long acted as chairman of the auroral committee of the International Association of Terrestrial Magnetism and Electricity (now the International Association of Geomagnetism and Aeronomy) of the Inter-

national Union of Geodesy and Geophysics. In this capacity he prepared (1930) a valuable *Photographic atlas of auroral forms*, giving a classification of forms, indicated by numerous photographs by himself and his assistants; it also described methods of photographic and visual observation of auroras. Later, in conjunction with la Cour, he prepared a *Supplement* to this *Atlas*. He was President of the Auroral Committee for the Second International Polar Year 1932/33.

8. SYNOPTIC AURORAL STUDIES

The parallactic auroral photographs gave not only the heights of individual auroral features, but also their location in geographical plan. Störmer published many synoptic charts showing the plan location of many auroral arcs and other forms shown on his photographs. He also published combined synoptic charts, showing the plan location of whole series of arcs observed by him over several years. He showed that the arcs are extended neither along the geographic nor the geomagnetic circles of latitude, but that they lie nearer the latter than the former.

9. SPECIAL AURORAL FORMS AND SUNLIT AURORAS

Störmer's studies of the heights of auroral points led him to provide valuable height-frequency diagrams, showing the height-range and height-distribution of auroras in general, and of many special types of aurora. His work forms the main basis of our knowledge in this field.

In the course of these photographic studies, in which he personally took a leading part, involving very many nights of long-continued work, he was quick to observe and distinguish auroras that departed from the commoner types. As a result he published many descriptions of special types of aurora, notable for their unusual form, movement, pulsation, or height. Among these fascinating researches, his discovery of the remarkable properties of sunlit auroras was the most prominent. It may be added that already in 1923 Vegard on theoretical grounds had inferred, and remarked to Störmer, that the upper limits of height of auroras should increase at lower latitudes (and he found observational evidence supporting this inference), and be greater at dawn and sunset twilight than at night.

In the great aurora of 22/23 March 1920 (of which he gave a most interesting description *The polar aurora*, pp. 8, 9) he found that in the earliest and latest periods of the night the auroral rays extended to very unusual heights, from bases at 250 to 350 km height, up to more than 800 km, much more than the auroral heights observed during the middle of the night. The significance of this difference was not realized until $6\frac{1}{2}$ years later, when he studied another remarkable grey-violet aurora (8 September 1926). It occurred to him that the remarkably high rays then also observed, after sunset, might be situated in the still sunlit high atmosphere; and the measurements from his photographs confirmed that this was so. He went

back to his 1920 photographs and found there the same result. 'The astonishingly high rays after sunset and before sunrise were sunlit, and the lower rays were in shadow' (p. 127). He pursued his studies into these remarkable phenomena, which, he states, when once seen are easy to recognize, from 'their colour, their length and their occurrence after sunset or before sunrise' (p. 131). 'The colour is mostly feeble white-violet, but it may also be red or blue' (p. 131). Sometimes there is a clear difference in colour between the sunlit and shadowed parts of the same auroral rays. Sometimes also a ray will be broken, a part being missing over a height interval that straddles the earth's shadow line. These and other results, illustrated by a number of well-conceived diagrams, form the subject of chapter 9 of *The polar aurora*.

10. THE COLOUR AND SPECTRUM OF THE AURORA

Störmer's descriptions of auroras give much information about auroral colours, of which he seems to have been a gifted observer. Though trained as a mathematician, his vital interest in observation led him to make spectroscopic studies of the aurora, but to a far less extent than did his colleague (and one-time assistant) Professor L. Vegard. *The polar aurora* includes a chapter (12) on the auroral spectrum, summarizing, though very briefly, the knowledge gained in this field, mostly by other workers, but also mentioning his own work, and that of some of his assistants.

It was Störmer's habit to carry with him a pocket spectroscope with which to examine the night sky, to seek for spectral evidence of the presence of aurora (even when the sky was cloudy), and to observe the main spectral features of coloured auroras.

He also kept by him the diagrams (prepared by J. Bartels) of the 3-hour K indices of geomagnetic disturbance, as a guide (based on the 27-day recurrence tendency) to the likelihood of auroral appearance on each evening.

11. AURORAL SOUNDS

Anyone who visits high latitudes is likely to meet 'old inhabitants' (and others) who assert that they have heard distinctive sounds during auroras. Usually they recall it as a rare happening, associated only with one or a few of the most outstanding auroras they have seen. Some scientists dismiss these stories as based on fancy or illusion, or as due to crackling of ice crystals frozen from the moisture of the observer's breath; they point to Störmer's own determination of the lower limit of height of the aurora—usually about 60 miles—and regard it as impossible that a phenomenon occurring so far above us should produce audible sounds.

Störmer himself did not enter into controversy on this subject. Apparently he, like the writer of this account, never himself heard the sounds. But he became convinced of their reality, from reports on two occasions of exceptional aurora: 15/16 October 1926 and 25/26 January 1938. The sounds

were heard by different observers (in 1938 by three persons) in whom he had confidence. He describes these cases on pp. 10 and 137-139 of *The polar aurora*. He added the comment: 'As the aurora on the two occasions was about 90 to 100 km overhead, the sound could not come directly from this, but must have been a secondary phenomenon: perhaps some sort of electrical discharge in the neighbourhood of the observers caused by the enormous amount of electricity accumulated in the region of the aurora. New observations of the electric potential gradient at the place of observation are needed to find if certain conditions were present, which do not accompany common aurorae of less intensity, even if the aurora is in the zenith' (p. 139).

Such speculations seem less improbable now that observations during the International Geophysical Year have revealed that ionizing X-rays associated with strong auroras can be detected down to levels of 20 miles or less. There is need for long continued audio and electrical recording in auroral regions, to gain fully objective records bearing on such reports.

12. LONG DELAYED RADIO ECHOES

Besides being alert to recognize new or specially interesting auroral features, Störmer was quick to note new phenomena that seemed to bear on his auroral interests. One chapter (15) of *The polar aurora* is devoted to what soon became known as 'wireless echoes of long delay'. On this subject he wrote eight notes or papers, the first two in 1928, four more in 1929, and two in 1930.

The story begins with a chance meeting in December 1927 between him and his neighbour Hals, an engineer who occasionally reported to him apparent auroral influences on radio reception. Hals told of strange echoes he had heard of signals from the Dutch station Eindhoven; the echoes had a delay of about 3 seconds. Hals suggested that they came from the moon. Störmer at once thought of another possibility, that they came from the electrons beyond the boundary of his 'forbidden regions'; their surface would vastly exceed that of the moon, and they would be like a concave mirror, instead of convex like the moon.

Störmer and Hals then began to study and record the echoes in detail. They concluded that the sounds heard were indubitable echoes of the signals; and van der Pol confirmed their conclusion. Special signals were emitted to further the investigation, and echo delays were recorded ranging from 6 seconds up to as much as 15 seconds. The wave length of the signals was about 30 metres. 'The signals were so powerful that they hurt our ears, and the echoes were also very strong, even though they were far from being as powerful as the signals' (p. 175).

The echoes disappeared completely at times, for weeks or months. 'This was a very depressing fact, but a closer study of the mathematical conditions for the appearance of the reflexion from electron surfaces gave a satisfactory explanation' (p. 177). Störmer predicted that the conditions for hearing

echoes would recur in mid-February 1929, and his prediction was verified: for example, Appleton and Barrow in London observed echoes with delay times up to 25 seconds. In May 1929 many such echoes were heard at a small station in Indo-China.

Störmer's explanation of the echoes was not universally accepted; van der Pol, for example, regarded them as a phenomenon of the earth's ionosphere.

After 1930 the Eindhoven station could not send further signals. During the years 1947-9, Budden and Yates at Cambridge sought to detect long-delayed echoes from a station in Essex, England (wavelengths 22 and 14 metres). No echoes were heard.

Recently Ratcliffe has stated that such echoes are never likely to be heard again, because nowadays the air is too full of radio signals.

13. OTHER LUMINOUS PHENOMENA OF THE ATMOSPHERE

Störmer's watch on the sky for aurora was occasionally rewarded by the observation of other interesting luminosities in the atmosphere. Among these were two kinds of cloud, occurring at unusual heights in the atmosphere.

One kind is called mother-of-pearl (or nacreous) clouds—a subject on which he wrote no less than seventeen papers from 1927 onwards until 1951. He applied his organization for determining auroral heights to the measurement of the heights of these clouds, and proved that they lie in the stratosphere, at heights from 21 to 30 km, well above the tropopause. They are thought to be formed of super-cooled water drops. They show beautiful iridescent colours, and in 1948 he presented colour photographs of them to the International Association of Meteorology, meeting at Oslo. He found that these clouds are associated with particular meteorological conditions (föhn), and that their occurrence in time was very irregular: Störmer saw them in 1890, but saw none between 1892 and 1926. He suggested that they may occur more often, but that in general lower clouds may hide them from view. The observation of these clouds is recommended to auroral workers as part of the programme of the International Geophysical Year.

Another type of cloud, occurring still higher in the atmosphere, bears the name *luminous night cloud*. These also Störmer observed, and again made good determinations of their height, which is about 80 to 85 km. Their nature is still uncertain. Störmer wrote eight papers on this subject between 1932 and 1942.

In 1911 he published photographs of the zodiacal light, and in 1929, 1939 and 1947 he wrote on meteor trails. His height-determining organization could not be applied to the study of such unexpected and fleeting luminosities as meteor trails, but he collected photographs that happened to be taken of certain trails. These he used to find their height and study the high-level winds indicated by the rapid distortion of the trails.

The *polar aurora* does not refer to the researches described in this section.

14. STÖRMER'S INTEREST IN PHOTOGRAPHY

In the preface to *The polar aurora* he remarked that 'it might be a source of interest to many to observe, in the development of my photographic work, what may result when a pure mathematician happens to be also an enthusiastic amateur photographer'. Like Chaucer's clerke of Oxenforde, 'gladly would he learn and gladly teach' in this favourite field of his endeavours. Besides writing on the photographic art as applied to auroral studies, he wrote also more generally on photography for amateurs, including photography of stars and of a solar eclipse.

In 1930 he told me how he developed into a photographer. When he was a young man at Oslo University he fell in love with a lady whom he did not know and with whom he was too bashful to become acquainted. Wishing at least to have a picture of her, he decided that this was possible only by taking a photograph of her himself, without her knowing. So he procured a small camera that could be concealed under his coat, the exposure being made through his buttonhole. He succeeded in this difficult task. The love affair came to nothing; later the lady left Oslo for America and married. Years later she returned to Oslo. By this time he was famous. Meeting her, he told her of the incident. Later he applied his skill in photography (un-suspected by the subject) to obtain pictures also of many of the celebrities of Oslo of those days. Long afterwards, when he was nearing the age of 70, these photographs formed the subject of an exhibition in Oslo. He published illustrated accounts of it (totalling over 60 pages) in 1942 and 1943—under the title (translated): 'Carl Johan (Street) snapshots of famous people of the last fifty years.'

15. OTHER SCIENTIFIC ACTIVITIES AND SUGGESTIONS

The form of the polar coronal plumes on the sun suggests that they may be associated with the lines of force of a magnetic field. With this in mind, Störmer in 1910 made calculations to find the trajectories in space of charged particles emitted normally from the sun, in a dipole magnetic field. In 1911 he wrote two papers on coronal structure, and on Arrhenius's theory of it. He spent some time as a Research Associate at Mount Wilson Observatory, California, in 1912. There Hale had recently crowned his discovery of sunspot magnetism by finding evidence of the sun's general magnetic field. This visit led Störmer to publish (in the *Astrophysical Journal* for 1912) a paper on solar vortices. He constructed wire models of some of his calculated trajectories of solar particles. They show much resemblance to the form of the solar corona at some epochs (p. 384 and Fig. 213, *The polar aurora*). In 1922 he attempted to estimate the pole strength of the sun's magnetic field, from the coronal structure.

In connexion with his calculations of corpuscular trajectories, he suggested that astronomers might well try to discover any luminous phenomena

resembling aurorae on the dark side of Mercury and Venus, especially during magnetic storms (*The polar aurora*, p. 361).

He wrote on a halo, in 1940. In 1923 he wrote in Norwegian a popular book that ranged from stars to atoms. It appeared in at least six languages; the French title is *De l'espace à l'atome*.

16. SOME PERSONAL CHARACTERISTICS AND ANECDOTES

I first met Störmer in 1921, when I was making a cycling visit to Norway. I found him to be a rather large and bulky slow-moving man. He gave me a friendly welcome to his office, to the Royal Academy of Norway (where he read a paper) and to his home, where his table was graced by many children. He was a thorough family man. At his death he left 5 children, 3 daughters-in-law, 2 sons-in-law, 15 grandchildren (3 of them married) and 5 great-grandchildren.

He liked the pleasures of the table, good food and wine; he liked the theatre and the opera, and took with him opera glasses the better to observe the action and the danseuses.

Once in my office in London he was visited by a known mathematician who wished to meet him. After she had gone he remarked to me that he had no idea that she would be so 'temptating'. From London he visited other universities and lectured there. He particularly enjoyed a dinner party after one of these lectures, and told me of one of his neighbours at table. Rather naughtily I asked if she too was temptating. 'No', he answered, 'not temptating, but cosy to talk to.'

He travelled much: to universities in other countries as a research student, to arctic Norway as an auroral photographer and investigator, to America as a research associate of the Carnegie Institution of Washington at Mount Wilson, and on at least three other occasions, to many foreign capitals and other cities to mathematical and geophysical congresses. He was an honoured and valued leader in international science in his chosen fields of research. His last long scientific journey was to an auroral symposium at London, Ontario, Canada; this was in 1951, shortly before he completed his 77th year. In later years he suffered serious illnesses.

As professor his rather exigent relationship with his assistants was far from that companionable ease, that level give-and-take, sometimes found in English university departments and more often in American. It is remarkable that none of his papers bears any name other than his own, though their text often refers to contributions by his assistants to their methods or ideas; some of those contributions were really substantial.

He made two visits to England, in 1930 and 1947, to give lectures in London, Oxford and elsewhere. He was a good lecturer, and his varied and fruitful researches provided ample material for his talks. In 1947, before his Halley lecture at Oxford, the honorary degree of D.Sc. was conferred upon him. He was photographed in his doctor's robes. Later—one small example

of his innocent delight in public recognition, and also of his meticulousness—he sent for a sample of the cloth of the robe, so that the photograph could be coloured with exactly the right shade.

The University of Copenhagen and the Sorbonne conferred a like honour upon him. In 1951 he was elected a foreign member of the Royal Society. He was a member of several academies of science in Scandinavia, and a Corresponding Member of the Paris Academy of Sciences, which already in 1922 had awarded him its Janssen Medal for his auroral researches. He was President of the International Congress of Mathematicians when it met at Oslo in 1936.

His marriage was long and happy. Mathematicians as husbands, with their reclusive habits of study, are not to the taste of every kind of wife. In his early married days, while he was studying in Paris, the only evening each week not dedicated to his studies was Tuesday. Later the pattern of his family life must have been considerably affected by his long nights' observations of the aurora, and by the watch he kept to seize favourable occasions. But as the preceding remarks will have indicated, he was very human, appreciative of family and neighbourly joys and the good things of life. He loved to amuse children and others with some tricks in which he was very adept. I particularly recall one in which he tied up a handkerchief to resemble a mouse. It seemed bursting with desire to escape from his restraining hand. Finally it succeeded in apparently running up his arm inside his sleeve, whence it was retrieved at the armpit from within his coat.

Dr Leiv Harang wrote, in a letter to me: 'Störmer always reminded me of the most lovable character in English literature: Mr Pickwick,—and when he lectured on his researches in auroras I always had to think of the scene in the *Pickwick Papers* where Mr Pickwick is presenting his report on his expedition to the ponds of London. I am thankful for having learned to know a man like Störmer; in our streamlined century a personality like his with all his peculiarities certainly never will develop.'

In 1950 Sir Edward Appleton, a General Editor for the Oxford International monographs on Radio, invited him to write a book on the aurora. He was then 75, and still active in auroral observation and study, though retired from his university chair. It was a tremendous task to undertake, but his acceptance and achievement of it was greatly to the benefit of his science. It finally appeared in 1955.

It is thus dedicated: 'To my wife ADA, who never ceased to encourage me to work hard till this book was safely finished.'

The book is limited in some directions, but it is and will be a classic in its field, worthy of the serious attention of all general students of the aurora. Its preface is written in the simple and modest terms worthy of a great scientist, such as he was.

In his reply to my letter of congratulation on its appearance, he remarked that everyone who engaged in the auroral programme of the International Geophysical Year ought to get a copy.

In the rooms of the Royal Astronomical Society in London there is a portrait of Gauss. Under the portrait there are the following words, put by Shakespeare in the mouth of the bastard Edmund in *King Lear*:

‘ . . . Thou Nature art my goddess,
To thy service I am bound.’

These words are as applicable to Störmer as to Gauss. Throughout his long life Störmer pursued his chosen course of observing Nature and applying his fine intellect to the unravelling of some of its mysteries. In the concluding paragraph of the preface to *The polar aurora* he says: ‘My work has given me infinite pleasure and satisfaction, but I regard it as no more than a pioneer effort.’

The photograph of Störmer facing p. 257 was taken when he was in his forties.

S. CHAPMAN

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Störmer's book *The polar aurora* gives abundant references to his theoretical and observational papers on that subject. A complete printed list of his papers up to 1944 was prepared by Knut Thalberg, and is obtainable from the publisher, A. W. Brøgggers Boktrykkeri A/S, Oslo. It contains 318 items (some of them multiple, referring to shorter notes). Unfortunately they are not serially numbered, as would have been convenient for reference. I have received a supplementary typed list from 1944 onwards; the items are numbered, and extend from 244 to 263, the last paper appearing in 1957. In the preceding text I have indicated the main series of his theoretical papers, and where they are to be found. The following list omits some of the shorter papers, including those on pure mathematics.

Theoretical papers on trajectories

Series A: In *Archives des Sciences physiques et naturelles, Geneva*; *Series 4*

1907, **24**, 5-18, 113-158, 221-247, 317-364.

1911, **32**, 117-123, 190-219, 277-314, 415-436, 501-509.

1912, **33**, 51-69, 113-150.

Series B: In *Videnskabselskabets Skrifter, Oslo, I, Mat.-naturv. kl.*

1907, No. 4.

1916, No. 6.

1913, No. 3.

1916, No. 12.

Series B¹: In *Videnskabselskabets Skrifter, Oslo, I, Mat.-naturv. kl.*

1912, No. 7.

1916, No. 5.

Series C: In *Videnskabselskabets Skrifter, Oslo, I, Mat.-naturv. kl.*

1913, Nos. 4, 10, 14.

1949, No. 2.

1936, Nos. 5, 6.

1950, No. 1.

1947, No. 1.

Series D: In *Zeitschr. f. Astrophysik*.1931, **3**, 31-52, 227-252.1932, **4**, 290-318.Series E: In *Avhandlingar, Oslo, Mat.-naturv. kl.*

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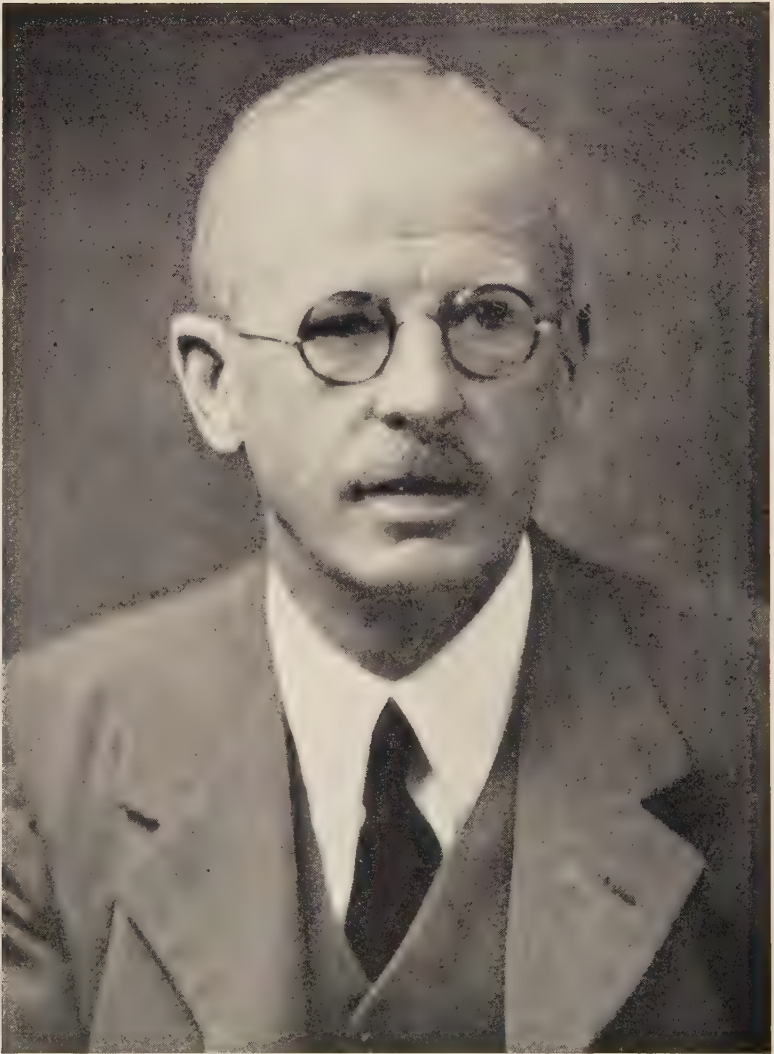
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Charles Todd

CHARLES TODD

1869-1957

CHARLES TODD died at Croydon on 22 September 1957, seventeen years after he had retired from active work. He was born on 17 September 1869 at Carleton, a small village near Carlisle. His father, Jonas Todd, was clerk and steward of the Cumberland and Westmorland asylum; his mother was born Grace Barker. His forbears on both sides of the family had been small farmers in that district as far back as records can be traced.

Charles was the eldest of the family: he had three sisters, two of whom died young. He had poor health as a boy and did not attend school regularly till he was 12. For a time before that he attended a dame school in Carlisle 'where the education consisted in the daily repetition of the multiplication table, the dates of the kings of England and the principal parts of the Latin verbs'.

At 12, he started at Carlisle Grammar School, where his career was undistinguished, maybe because the teaching consisted almost entirely of 'the drudgery of classics', with some mathematics. His real interest was in natural science, 'hardly regarded as a serious study', though there was apparently a small chemical laboratory.

Despite what would appear to be hopelessly unfavourable circumstances, he obtained an open scholarship in chemistry and physics at Cambridge and went up to Clare College in 1888. In due course he obtained a 'first' in Part I of the Natural Science Tripos, and had his scholarship extended so that he could stay up for another year; he obtained second-class honours in Part II of the Tripos. In 1892 he gained another open scholarship in physics and chemistry, this time at St Bartholomew's Hospital. Nothing is recorded as to his period at Barts, except that he qualified (M.R.C.S., L.R.C.P.) in 1894 and later took his Cambridge M.B., presenting a thesis on 'the treatment of septic and lacerated wounds'; this was based on his experience at Addenbrooke's hospital where he was appointed, immediately on qualification, house-surgeon under Sir George Humphrey.

In the same year he developed bronchitis following influenza and because of an unsupported suspicion of tuberculosis was advised to take a sea trip. So he went in 1895 as ship's surgeon to the Cape and later to Brazil, the West Indies and New York and returned to England full of strength and vigour.

He next became assistant medical officer to the London Fever Hospital, became interested in public health and took his Cambridge D.P.H. For his M.D. he presented a thesis on a diphtheritic nasal infection in scarlatinal

convalescents (2).^{*} 1898 and 1899 cover what must have been a frustrating period. He went out to Beira in Portuguese East Africa, hoping for opportunities for research on malaria, which was rife amongst workers engaged in railway construction. The hoped-for facilities were not available and he returned to England in 1900 as soon as his contract was over. He was then appointed assistant bacteriologist to the Lister (then the Jenner) Institute. At Sudbury, near Harrow, antitoxin was being produced in a small two-roomed building with stables adjacent. Here Todd worked as assistant to the Director, George Dean: they had personally to inject and bleed the horses and bottle the sera. Some time was nevertheless available for research: experiments initiated by Lord Lister were carried out to test Koch's statement in 1901 that cattle should not be considered a dangerous source of infection of human beings; Koch's ideas were not supported (7). Todd also found that some strains of *B. megatherium* produced a potent haemolysin (4, 5), which he used to follow up some work by Ehrlich and Madsen on toxin-antitoxin reactions; this proved a practicable model, more convenient than the then available methods for working with tetanus and diphtheria toxins.

In 1902 the serum laboratories were moved to better accommodation at Elstree, and G. F. Petrie joined the staff. Todd now began to work on Shiga dysentery and was able to produce a potent toxin by growing the bacillus in alkaline broth and to elicit a good antitoxin by immunizing horses (8, 9). This became a regular issue from the Elstree laboratories.

In 1904 a serious outbreak of cattle plague (rinderpest) had broken out in Egypt and 1000 cattle were dying every day. To meet an urgent demand for antiserum, an establishment had been hastily set up near Cairo, but this was not functioning at all well. Todd was asked to re-organize and direct it. Thus began an association with Egypt which lasted for 21 years.

The state in which Todd found the Serum Institute is best described in his own words. 'Inspection of the Institute revealed an astonishing state of affairs. Erected on the summit of a wind-swept hill in the desert a few miles from Cairo, it consisted of matting stables and sheds surrounded by a ring fence which, at that time, was being patrolled at night by an armed guard, owing to a threat of the local butchers to set fire to the place. Of the 250 native bulls supposed to be producing serum some 100 were suffering or recovering from abscesses of various sizes. Foot-and-mouth disease was rampant and there was much protozoal infection. The only water supply consisted of unfiltered Nile water pumped up the hill by a very primitive hot air engine and all steam sterilization had to be done by means of primus lamps. As each reinforcing injection of a serum-producing animal consisted of 4 litres of virulent blood, given subcutaneously, and as this blood had been whipped in uncovered enamel dishes in the open air and filtered through muslin, often in a dust storm and in the presence of innumerable flies, the occurrence of abscesses was hardly to be wondered at.'

^{*} The numbers in parentheses refer to the numbered entries in the bibliography at the end of the paper.

It says much for Todd's organizing ability that within six months the Institute was working normally (15). With reduction in the incidence of cattle plague and accumulation of good stocks of serum it was possible in September of the following year (1905) to close the Institute down. As work at the Institute diminished, Todd was able to spend his mornings at the Hygienic Institute in Cairo, where he incidentally carried out some work on fowl-plague and barbone (a haemorrhagic septicaemia of buffaloes) and spotted the occurrence of trypanosomiasis in camels. When the Serum Institute closed he was drafted as bacteriologist to the Hygienic Institute where for some time his duties were mainly of a routine nature, 'chasing plague rats and fleas'.

In 1906 he began studies of scorpion venom with a view to producing an antiserum; many deaths were occurring annually especially amongst children. Offer of a small reward brought in a supply of some 3000 scorpion tails which were dried over calcium chloride till they could be used. The work went well and culminated in a paper on 'an antiserum for scorpion venom' in 1909 (11) and in arrangements for making the antivenom on a larger scale at the Lister Institute.

Between 1906 and the outbreak of war in 1914, Todd was busy with work of various kinds in the field of hygiene. The Institute's Director, Professor H. Bitter, was away for a time on leave and later on long sick leave and Todd acted as Director. During one of these periods 'the scandal of the Infectious Diseases hospital at Abbassia was tackled. The hospital there had no drinking water supply, no drains, no nurses and the roofs of most of the pavilions were leaking. With the aid of a grant of £2000, obtained with much difficulty, the hospital was put into a reasonable working condition'. Other work concerned cholera in Mecca, Malta fever, contaminated water supplies at Khartoum, louse transmission of typhus and relapsing fever and filariasis (19).

On the outbreak of war, Todd, on sick leave in England, was recalled to Egypt and shortly afterwards succeeded Bitter as Director of the Hygienic Institute, of which the title was changed to that of the Public Health Laboratories. Much help was supplied to the British army in Egypt, and later, to centralize the work, the Institute was constituted the Central Bacteriology Laboratory for the army in the Cairo district. After the war, the activity of the Nationalist movement and the declaration of independence of Egypt made things very difficult for Britons there. In 1922 Todd sent in his resignation but was persuaded by the Egyptian Government to remain in a consultant capacity; he finally returned home to England in May 1925. How arduous were his routine duties is shown by the presence in his bibliography of only four papers published between 1914 and 1927.

Throughout his twenty-one years the problem of cattle plague was recurrent. The Serum Institute was re-opened in 1907 under R. G. White, with whom Todd was to collaborate so closely. In 1912 and 1913 Todd and White were members of a Commission appointed to study cattle-plague and were asked to draw up a programme for further work. This involved considerable extensions to the Serum Institute. The resulting work occupied most of

Todd's time for two years; it was concerned mainly with practical measures for dealing with the epidemic. The conclusions were published in detail in 1914 (17). Hyper-immunization of cattle involved injections of large quantities of blood from other infected cattle. In 1910 Todd and White discovered that these procedures led to production of isolysins, antibodies capable of lysing the red blood cells of other cattle (12). The resulting studies on cellular individuality were continued over many years. They afford Todd's principal claim to scientific distinction, and are considered, along with his subsequent work on iso-agglutinins in fowls, later in this notice.

RETURN TO ENGLAND

Early in 1926, not many months after his return to England, Todd was asked by the Medical Research Council to visit Italy and later Germany to investigate claims to have cultivated viruses from measles and scarlet fever; these claims have not since been substantiated. Following these visits, in May 1926, Todd was offered a research grant to take part in a programme of work on viruses at the National Institute for Medical Research at Hampstead. This had been initiated a few years earlier in the Department of Pathology under Captain S. R. Douglas. Laidlaw and Buxton, and later Dunkin, were working on dog distemper, Gye on fowl tumours, Perdrau on herpes simplex, Barnard on ultra-violet microscopy. Here Todd turned his attention to fowl plague, a virus he had studied in his earlier years in Egypt.

It is not everyone who, at the age of 57, after many years of heavy administrative commitments, can turn again to research and make important discoveries; yet Todd achieved this.

In the field of fowl plague, he showed (23) that, as with cattle plague, virus in the blood is located within the leucocytes. The mechanism of contact infection in fowl plague was obscure: Todd showed (23) that the virus could traverse intact mucous membranes and that sexual intercourse was at least one means of transmitting the infection. With W. J. Elford he measured the size of the virus (31). Earlier attempts at artificial immunization had proved fruitless, but Todd (24) was able to produce immunity to fowl plague by means of a phenolized vaccine made from livers of infected birds. Birds so immunized and then hyperimmunized yielded an antiserum. This, though potent in some experiments, gave irregular results. For instance, large doses of a serum + virus mixture given intravenously might prove innocuous while a much smaller dose of the mixture would kill. It was found that a just-neutral mixture could be rendered virulent by simple dilution with saline; Todd concluded that the action of the antiserum was not simply a matter of destroying the virus *in vitro*. He published an account of this 'dilution phenomenon' (25)—or the 'Todd phenomenon' as we used to call it at Hampstead. Bedson's (1928) experiments with herpes simplex and my own with vaccinia (Andrewes 1928) gave similar results at about the same time. The phenomenon has proved since to be of importance in understanding the

mode of action of antiviral sera. Once this matter was cleared up Todd was able to immunize fowls by simultaneous injection of fowl plague virus and antiserum.

At about this time he studied the electrical behaviour of bacteriophage and designed a simple cataphoresis apparatus for the purpose (22). He was a pioneer in this field of investigation. At one period of his time in Egypt he had had to give instruction in physics; so physical aspects of virus research always interested him. So we find that the next subject to excite him was the photo-dynamic action of methylene blue on viruses. He began with bacteriophages, for Clifton (1931) had shown that the action of methylene blue on a staphylococcal phage was essentially a process involving oxidation, occurring only in the presence of light. This observation Todd, working with J. R. Perdrau (33), confirmed and extended; he was particularly interested to find that the phage became very much more resistant after union with sensitive bacteria; these, if they were to evince their protective action, had to be living.

He then collaborated with Perdrau in showing that the action of methylene blue on several viruses was of a similar nature (34). Viruses, however, differed in sensitivity, the smallest, those of foot-and-mouth disease and poliomyelitis, being relatively resistant. As with bacteriophage, viruses such as herpes and vaccinia were protected against inactivation once they had gained the shelter of a susceptible cell (37). Some photodynamically inactivated viruses proved to make efficient vaccines. Dog distemper virus was the first one to be studied; such a vaccine proved effective in ferrets but gave irregular results in dogs (32). A vaccine of this type was ineffective against poliomyelitis. This work has not been further followed up.

The most important contribution made by Todd during his period at Hampstead was his work on iso-agglutinins in fowls, a continuation of the studies in Egypt on isolysins in cattle. I am indebted to Dr D. G. Gilmour for the following review of Todd's classical work in the field of cellular individuality.

'Todd's interest in the individuality of the red blood cells of animals was first aroused while he was working at an Institute in Egypt for the production of immune serum against rinderpest (cattle plague). He and R. G. White realized that the injection of infected cattle blood into other cattle for this purpose might have also stimulated the production of immune isolysins for cattle red cells. Out of 106 sera from cattle which had been so injected, they found (12, 13) that 102 strongly lysed the cells of many other cattle, after addition of fresh guinea-pig serum to provide complement. In confirmation of Ehrlich & Morgenroth's (1900) findings with goats they found a wide variety of individual differences both in the reactivity of cells and in the response to injection. Thus each individual serum lysed some but not all cells, and those lysed differed with each serum. Moreover, the same blood injected into different individuals produced sera of differing reactivity patterns. Reasoning from these findings, they concluded that by pooling a number of immune sera they would produce a highly polyvalent serum

reacting with all cells, and if such a serum were absorbed with cells of any individual it would still react with all other cells and thus act as a highly specific reagent for this individual. Such was the case. Sera from 60 to 70 immunized cattle were pooled, and tested against cells of 110 other cattle, with the results they had predicted. Only in one case did the pooled serum absorbed with cells of one individual fail to lyse the cells of another individual. The absorbing cells came from a cow and the non-reacting cells from her calf. This finding in 1910 was the first indication that these individual characteristics of red cells in animals were inherited. In claiming that there was a multiplicity of individual red cell antigenic differences among cattle they foreshadowed both the later findings of American workers with cattle and the then unrealized complexity of the situation with human blood groups.

'When Todd returned to England he was eager to continue this work, but for some time was unable to obtain the large numbers of animals required for the production of sufficiently polyvalent sera, because the usual small laboratory animals appeared unsuitable for isolysin production. During the preparation of an anti-fowl plague serum he stumbled upon the fact that injections of fowls with red cells from other fowls produced powerful iso-agglutinins, and he proceeded to repeat his earlier classical work with this material. Once again a wide variety of individual differences was found in cell reactivity and antibody response, while a highly polyvalent pooled serum when absorbed with cells of each individual in turn still agglutinated cells of all the other 89 fowls tested (26). Definite proof that the differences were inherited as simple Mendelian dominants was obtained when it was found that the polyvalent serum absorbed with blood of one parent of a family of chicks failed to agglutinate cells of 20 to 30 per cent. of the offspring, but when absorbed with blood of both parents invariably failed to agglutinate cells of all offspring.

'In a later paper (27) Todd gave the results of further work with three families. The polyvalent serum was absorbed with each member of a family in turn and tested against all others. No two bloods within a family gave exactly similar results, although some were very closely alike. He later showed (36) that by inbreeding from brother-sister pairs of very similar reactivity it was possible to produce three generations of fowls all almost or completely identical in their red cell reactivity with the serum.

'Todd's main conclusion, that the red cells of fowls possess a multiplicity of antigenic affinities or receptors which are inherited as dominants, was questioned by several workers in the years immediately following his publications, although it has since been amply confirmed by later more detailed work. Thus Kozelka (1933*a, b*) on the basis of absorption work with antisera made in rabbits, concluded that relatively few antigens existed, and that the apparent multiplicity of differences reported by Todd could be explained by a variable assortment of a limited number of factors. Wiener (1934*a, b*) drew the same conclusion from a re-analysis of Todd's own results. Wiener's

analysis is open to criticism, for in order to make the interpretation more clear-cut he was prepared to count a number of powerful positive reactions recorded by Todd as negatives, writing them off as "a small percentage of inconsistencies". Later work has, however, shown that very numerous separately inherited antigens do exist in fowls, and the fact that they can be variably assorted in combinations merely increases the number of separate individualities towards infinity. Thus Briles and his co-workers in America have described five loci determining red cell antigens in fowls, each comprising numerous multiple alleles. At their *B* locus, for example, at least 21 distinct alleles have been found (Briles, Allen & Millen 1957). Gilmour (1958) in this country has found further alleles at each of these five loci, and in addition has reported two more loci. In all this work sera prepared in the chicken rather than in other species have been used, a point which Todd realized to be of the highest importance.

'Thomsen (1934, 1936) also questioned Todd's other finding, that the factors were inherited as dominants, by apparently identifying antigenic factors in offspring which were not present in either parent. He concluded that some antigens were inherited in either a recessive or a complementary fashion. Briles (1949) repeated Thomsen's experiments and was able to show that an error of serological technique had been involved, and that there was no evidence for recessive or complementary inheritance.'

In 1935 Todd's M.R.C. research grant terminated under the age limit but he continued, as a voluntary worker, to study photodynamic inactivation of viruses with Perdrau and in various other investigations which had no important outcome. He gave up regular work at Hampstead in July 1937.

On the outbreak of war in 1939 Sir Henry Dale invited him to join a team at Hampstead under Sir Patrick Laidlaw engaged in studying problems of air hygiene. This work had been initiated by Laidlaw largely because of the fear of serious epidemics in crowded air-raid shelters. With Elford, Todd studied the properties and persistence in the air of bactericidal mists and their action on viruses and bacteria suspended in the air. Todd's contribution lay particularly in his skill in devising appropriate techniques. He continued with this until, in October 1940, air-raids made travel between Sutton, south of London, and Hampstead, increasingly difficult. On one day almost seven hours were spent in train, tube, bus and taxi: so the work was reluctantly discontinued.

When I knew Charles Todd in his years at the National Institute for Medical Research at Hampstead between 1927 and 1940 he was always a delightful and helpful colleague. He was well over six feet tall, loosely knit, with a characteristic, almost shambling gait, his face nearly always lit with a ready smile. He had an enormous gift for enthusiasm; each new avenue of approach in his research was 'tremendous fun'. Withal, he was extremely modest about his own contributions and very critical, almost cynical, about surprising new claims. 'Very interesting, if true' was a common remark and we used to call the saying 'All men are liars' the Law of Todd and David.

He was interested in and knowledgeable about all sorts of natural phenomena outside his own work. He was full of amusing anecdotes especially about his years in Egypt. His funds of knowledge and ideas were always freely at the disposal of his colleagues. He was well versed in medical literature and continued as a valuable member of the Institute's Library Committee until 1950.

During his years in Egypt he naturally served on many Commissions and Committees. After his return home he was a valued member of the joint Colonial Medical Research Committee of the Foreign Office and Medical Research Council (1927), of the Foot-and-Mouth Disease Committee (1930) and of the Research Advisory Committee of the Empire Rheumatism Council (1937).

His distinctions include the award of the O.B.E., of the Order of the Nile (2nd class) and Order of the Medjidieh (3rd class). He was elected F.R.S. in 1930. He never married. He had shared a flat in Cairo from 1910 till 1915 with his colleague R. G. White, and even after White's marriage in 1915 they were still with each other a great deal. After Todd's return to England in 1925, he made his home with the Whites at Sutton in Surrey, travelling to Hampstead every day. On his final retirement in 1940 he took over the care of the garden, considered himself as plumber to the establishment and took a keen interest in various aspects of natural history; he also had a laboratory bench fitted up in the house.

Charles Todd's friendly yet forceful personality, his enquiring mind, his critical and keen brain made him one of those who are outstanding both in the fields of administration and research.

For the material in this notice I have drawn very freely on the personal record left by Todd himself. Following the Bibliography are references to relevant work by others.

C. H. ANDREWES

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A. E. Tinsman.

ARTHUR ELIJAH TRUEMAN

1894-1956

ARTHUR ELIJAH TRUEMAN was born on 26 April 1894 at Nottingham. He was the son of Elijah Trueman and Thirza Cottee, who were both natives of Nottingham. He lived at various places near the borders of Nottingham which were always within easy reach of the country and he recorded that at an early age he was particularly interested in sketching from nature; this facility he retained throughout his life, many of his papers and books being illustrated by his own sketches and drawings. In later years, he was interested and adept in water-colours, especially landscapes, which gave pleasure to him and to his friends.

In 1906, he gained a scholarship to High Pavement School, an old foundation established as a City Secondary School in Nottingham and he remained there for five years under the headmastership of Edwin Francis; before he left he had passed the Intermediate B.Sc. Examination of the University of London; it is indicative of his special interests at this time that he asked for a microscope as one of his prizes. The Field and Camera Club of the school exerted an important influence upon Trueman; he organized field excursions and took an active part in exhibitions of natural history specimens. He also became secretary of the Nottingham branch of The Young Naturalists' Association instituted by Percival Westall and visited other schools in the city on behalf of the Association. He took up the study of variation in the shell of the common banded snails, systematically collecting these shells and making careful distribution maps. This work resulted several years later in a short but interesting paper which is based upon a very large number of specimens. His interest in the variation in the form of shells and his assiduity in collecting them, which were to remain unabated throughout his life, were thus developed whilst he was a boy at school.

He left High Pavement School in 1911 and became a student teacher at Huntington Street School in Nottingham, where he said that he was first shown how to teach by the headmaster; here perhaps and afterwards at University College, Nottingham, were laid the foundations of his outstanding success as a university teacher in later years. He entered University College, Nottingham, in 1912; no scholarships were then available from his school and he held a grant as a teacher in training; the training was done concurrently with degree work since he began at once to read for the B.Sc. degree of the University of London. It was Trueman's intention to take zoology as his main subject for honours but at about the time of his entry to the College, Professor H. H. Swinnerton had been appointed to the Chair

of Geology and his department separated from the former Natural Sciences Department. Trueman became a student of geology with Swinnerton and zoology was taken as a subsidiary subject in his degree course. At this time, Swinnerton was preparing the material for his well-known book, *Outlines of palaeontology*, and Trueman rapidly acquired a liking for the biological aspects of palaeontology as might have been expected in view of his special interests and aptitudes whilst at school. The influence of Swinnerton was particularly important in these formative years and was manifested subsequently in Trueman's researches, in which he was always deeply concerned with the principles and concepts of palaeontology. He was an exceptionally brilliant student and his theoretical studies were reinforced as they were throughout his life by extensive field work, at that time particularly in the country around Nottingham. In 1914, he graduated with first class honours in geology with palaeontology as his special subject; earlier in the year he had obtained his Teacher's Certificate.

From 1914 to 1917 Trueman was a research student with Swinnerton at Nottingham and held various research grants; he also acted as a Student Demonstrator and in addition gave a course of lectures in geography. Before graduating, he had commenced work on the Lias in the area south and east of Nottingham and had carefully studied the fossils which he had collected; a short paper was published in 1915 soon after graduation. He was awarded the M.Sc. degree of the University of London in 1916 for a thesis on the morphology of the ammonite septum. He continued his researches on the Liassic rocks and fossils and in 1918 he was awarded the D.Sc. degree of the University of London for a thesis dealing with the systematics and evolutionary development of the Liparoceratidae.

At the beginning of the 1914-18 war, Trueman volunteered for active service but was rejected on medical grounds; later medical examination showed that he was unfit for military service and in 1917 he was freed from further obligations. When Trueman took his B.Sc. honours examination in 1914, Franklin Sibly was one of his examiners and from that time took a great interest in his work. Swinnerton records that in 1917 Sibly wrote to him saying that he had been so impressed with Trueman's papers in his honours examination that he would like to have him as a member of his staff in the Department of Geology at University College, Cardiff. Accordingly in the autumn of 1917, Trueman took up duty as Assistant Lecturer at Cardiff under Sibly and he also served for a short time with Professor A. H. Cox, who was Sibly's successor in the Chair of Geology. Thus began a close association and friendship with Sibly which continued until Sibly's death in 1946. Trueman's early interests had been essentially palaeontological; he ascribed the extension of his interests to stratigraphical problems to his association with Sibly.

Sibly left Cardiff to be Principal of University College, Swansea, and Trueman joined him as Lecturer and Head of the Department of Geology when the College was opened in 1920; he was then 26 years of age. He was

one of the first six members of the Senate of the College and during the thirteen years he remained in Swansea, he played an important part in the early development of the College. He had the great task of starting an entirely new Department of Geology involving not only degree courses in geology but also courses of study appropriate for civil engineers and metallurgists. Furthermore, within a year or two after arriving in Swansea, a request from local teachers for instruction in geography was enthusiastically taken up by him and for a time he held classes on Saturday mornings leading to a Certificate in Geography; this was the beginning of a growing development in geography at Swansea, which matured as a full degree course in 1929; Trueman thus initiated two departments in Swansea, Geology and Geography, the latter subsequently separated from the former. He was without assistant staff in the early years at Swansea and his burden of teaching must have been exceptionally heavy but he actively continued his researches on the Jurassic rocks and fossils and began his great work on the Coal Measures which was to be his primary research interest for the rest of his life; and with all these commitments and responsibilities, he found time to write papers on problems of local geography and to collaborate in the preparation of a book on the geography of South Wales. He was appointed Professor of Geology and Head of the Department of Geography in 1930; by the time he left Swansea he had established a flourishing department and research school.

Trueman was appointed Channing Wills Professor of Geology in the University of Bristol in 1933 and there he continued his teaching and research with unabated vigour. And as at Swansea, he took an active part in the general work and life of the university, serving as Dean of the Faculty of Science for three years before he was invited to the Chair of Geology in the University of Glasgow in 1937. He stayed in Glasgow until 1946, preoccupied for six years with many wartime activities. He was again just as active and successful in teaching and research; and as elsewhere he took a full share in the general administrative work of the university, serving for the last two years as Deputy Principal in close association with Sir Hector Hetherington, Principal of the University. Furthermore, he had many commitments outside the university; for example, from 1943 to 1946 he was a member of the Elliot Commission on Higher Education in West Africa, travelled widely in the West African Territories and in addition to his duties as a member of the Commission found time to see much of the mineral industries, the local Geological Surveys, and doubtless to add to his store of geological knowledge.

Trueman left Glasgow in 1946 upon his appointment as Deputy Chairman of the University Grants Committee. He became Chairman of the Committee in 1949 and remained in that office until he resigned because of ill-health in 1953. Even with his new commitments and heavy responsibilities, he was still devoted to his geological studies. He remained in close and intimate touch with his old students, former colleagues and co-workers in his special fields of interest; and he always appeared to have time to give to his scientific

activities. He was President of the Geological Society of London from 1945 to 1947 and delivered two memorable addresses; he was President of Section C (Geology) at the Brighton meeting of the British Association in 1948 as well as in the same year attending and renewing personal contacts at the meetings of the International Geological Congress which was held in London; he continued as Chairman of the Geological Survey Board until 1954 even after he had retired from the University Grants Committee, and in the last years of his life, despite grave physical disabilities, edited and contributed to an important book on the coalfields of Great Britain. Those who had known him throughout his academic career and were in close contact with him during the last ten years of his life in London were amazed by his remarkable energy, resilience and determination, which remained practically unabated until gradually his physical powers waned and he passed peacefully away from us on 5 January 1956 at the early age of 61.

Trueman was a great teacher of geology and of many allied subjects such as geography. He began teaching at a very early age; for nearly thirty years, he carried a heavy burden of teaching in several universities and all his students pay tribute to the stimulus and excitement which his lectures and demonstrations aroused in them; they remember with gratitude his grasp of his subject and the clarity of his exposition. He was not only the teacher of his students; he was in a very special way their personal friend, securing their devotion and affection. Many generations of students were inspired by his teaching and research; many of them occupy important scientific appointments including professorships of geology. Those who were not his students equally remember him in learned societies and elsewhere; his sense of humour, his incisiveness and his mastery of his subject were evident to everyone; it was a high privilege to share his friendship and to receive assistance and generous encouragement from him.

Furthermore, he was keenly interested in the popularization of science and in particular of geology. He was for a time a university extra-mural teacher of adult classes; he was ever ready to help local amateur societies; and he was active in encouraging the teaching of geology in schools. He wrote books appropriate for pupils in schools and his text-book, *An introduction to geology*, is widely used. He was in the post-war years Chairman of the British Association Committee on the Teaching of Geology in Schools and in his presidential address to the geology section of the Association in 1948, where he was taking stock of the present and future of geological science, stressed the value of geology as a cultural subject in schools and universities. He also wrote a book more especially for the layman on the scenery of England and Wales, which was later issued in a popular edition.

Trueman was an active member of scientific societies both local and national. He was a strong supporter of such societies in all the university centres where he worked, for example, in Bristol he was President of the Geological Section of the Bristol Naturalists' Society and in Glasgow he was President of the Glasgow Geological Society. He was a member of the

Geologists' Association, assisted in leading field excursions to the Cardiff and Gloucester districts and contributed to the Proceedings of the Association. He was a Fellow of the Geological Society of London from 1917, served as a member of its Council for a total period of fifteen years, was a Vice-President for five and President for two years. He regularly attended meetings of the British Association; in 1952 he was appointed a General Secretary of the Association but failing health prevented his continuance in that office. His influence was thus widely felt in schools and universities, in learned societies and in the general world of science.

Trueman's earliest researches were concerned with the Liassic rocks and fossils of the Nottingham district and in them he owed a great deal to the guidance and stimulus of Swinnerton. His first paper published in 1915 contained a description of the lowest Liassic rocks in the south of Nottinghamshire. This work was extended farther afield and in 1918 he described a full Liassic succession in south Lincolnshire, discussing in detail the lithology and fauna; variations in the thickness of the Upper Liassic rocks were ascribed to uplifts during deposition and the migration of the area of maximum deposition from north to south. He was at the same time beginning his researches on the ammonites; early papers revealed the author's interest in the systematics of these fossils and the use of detailed morphology in ontogenetic studies and lineage so characteristic of his later work; in 1917, with Swinnerton he discussed the morphology and development of the ammonite septum in three selected species. This was followed by a short account of the ammonite siphuncle and in 1920 he published an important paper on the evolution of the Liparoceratidae, which, in addition to dealing with the general evolution of these Lower Liassic fossils, also contains a systematic study of genera and species and their development. The influence of Buckman and others was manifest in this work on the Liassic rocks and fossils as it was in subsequent studies by Trueman. These investigations led to another paper in 1920 in collaboration with A. H. Cox on the intra-Jurassic movements in the southern Midlands as revealed by non-sequences and reduced thicknesses of Lower Jurassic zones along anticlinal axes and contained a discussion of the possible bearing of these flexures upon the structure of the underlying Palaeozoic floor.

Trueman's appointment to the staff of the Department of Geology at Cardiff provided an opportunity for a study of the Liassic rocks of South Wales; and this was continued after he went to Swansea. He was at first concerned with the Liassic rocks of the Cardiff district upon which he published in 1920. The general sequence was worked out in the cliffs at Penarth but the paper was mainly concerned with the Liassic outliers near Cardiff; he had not then completed his investigations in the area between Barry and Bridgend. The full account of the Lias of Glamorgan was published two years later; both the normal and littoral facies of these rocks were described in detail; close attention was given to the ammonites and to the lamellibranch *Gryphaea* in the classification of the strata; and much additional

information was gained about the interesting palaeogeographical conditions which prevailed in this part of South Wales during Liassic times. Trueman showed his continued interest in the Liassic rocks of this area by publishing several years later a detailed account of the lithology and fauna of the bucklandi zone at Nash Point, Glamorgan.

It was natural that Trueman should extend his researches to the very variable Liassic rocks across the Bristol Channel in the Mendip region and this occupied part of his time in the early years at Swansea. With J. W. Tutchter, he published a paper in 1925 on the Liassic rocks of the Radstock district in Somerset in which a careful study was made of the sequence and distribution of fossils. Attention was drawn to the condensed development of certain zones and numerous non-sequences were demonstrated. These non-sequences are marked in several instances by beds of phosphatized nodules with 'derived' fossils but at other levels important non-sequences are revealed by palaeontological evidence without any other indication of a stratigraphical break in the succession. The nature and effects of intra-Liassic movements were traced and the history of the area in Liassic times was discussed. The variations in the thicknesses of certain divisions were explained by intra-Liassic folding and penecontemporaneous erosion; the authors were able to demonstrate the importance of posthumous Armorican movements upon sedimentation in Liassic times.

Meanwhile, Trueman was continuing his studies in palaeontology and in 1921 he published a paper in collaboration with A. I. Macdonald on the evolution of Liassic gastropods. The ammonites had proved to be the most useful fossils in the zoning of the Jurassic rocks but they are rare in some divisions; this investigation was undertaken to assess the value of the Liassic gastropods in correlation. The authors were concerned mainly with two families, the Procerathidae and the Loxonematidae. It was shown that the evolution of these gastropods proceeded more slowly than that of the ammonite families and therefore correlation by means of them was more difficult. However, certain series of the Procerathidae evolved fairly rapidly and some species of such series characterize definite horizons; it was suggested that if care is taken in identifying species such forms might prove useful in correlation; this emphasis upon precise identification was characteristic of all Trueman's palaeontological work and was to prove so valuable in his researches. There followed in 1922 his well-known paper on the use of the lamellibranch, *Gryphaea*, in the correlation of the Lower Liassic rocks. During his investigations of the littoral facies of the Lias in South Wales and in parts of Somerset, Trueman had often found that almost the only fossils in some of these deposits were species of *Ostrea* and *Gryphaea*, which occurred in regular sequence and were of considerable value in correlation. It had been generally accepted by palaeontologists that species of *Gryphaea* had evolved from *Ostrea* by shortening of the period of attachment and the arching of the left valve. Trueman made a detailed study of the development of *Gryphaea* through several hundred feet of Liassic strata in the Vale of Glamorgan; he

showed that this represented a true genetic series or lineage and that *G. incurva* probably evolved through innumerable intermediate stages from oysters of the type of *Ostrea liassica*. The lineage of *Gryphaea incurva* ranges from the Oyster Beds at the base of the Lias where it is represented by flat, oyster-like forms to the bucklandi zone where it is typically represented by very incurved forms. The paper contained notes on species and a statistical study of many forms; graphical methods were used to show the proportions of specimens at each horizon in the various stages of coiling. This was a particularly elegant and fruitful investigation; out of it came important contributions to the principles and concepts of palaeontology.

Trueman was also at the same time continuing his work upon the ammonites, for example, in 1922, he discussed ontogeny in the study of ammonite evolution. It is well known that the development of an individual generally recapitulates to some extent the stages passed through during the evolution of its ancestors. Palaeontologists had recognized the importance of skipping of stages in development; Trueman showed that the study of fossil lineages afforded a means of investigating the ways in which skipping of stages had been brought about and he showed that this was particularly true in the case of the ammonites amongst which it is comparatively easy to study the ontogeny of successive members of a lineage. This was followed in 1924 by a discussion of the species concept in palaeontology. He suggested that any natural classification must be based so far as practicable on genetic affinity, that a palaeontological classification must be of such a character that a name or symbol is available to designate every recognizable form which characterizes a distinct horizon, and that in many respects it is better to have too many names or symbols than too few. He discussed species in relation to lineage and considered that lineage is rather a bundle of lines or more correctly a plexus of lines which repeatedly branch and unite and that in such a lineage there is a definite trend in the progressive characters but also considerable variation in the characters at each horizon. In 1925 with D. M. Williams, he studied the ammonites of the family *Echioceratidae*, which as in the case of his work on the *Liparoceratidae* contained a systematic study of genera and species and details of development; in all these he used biogenetic principles and assessed the relations of ontogeny and phylogeny. Trueman's interest in the Jurassic rocks and fossils was maintained throughout his life and many years later after he had moved to Glasgow he published papers on the body-chamber with reference to the buoyancy and mode of life of ammonites, and on the Liassic rocks of Skye; but a new interest had already manifested itself, namely the stratigraphical palaeontology of the Coal Measures which henceforth was to be his main task and for which he will be especially remembered by palaeontologists and stratigraphers.

However, before turning to his work on the Coal Measures, mention should be made of his geographical studies, which were undertaken particularly during his time at Cardiff and Swansea. As early as 1919, he published a paper on population changes in the South Wales Coalfield and this was

followed in 1921 by another on the iron industry of that region. He also wrote on the physical features of South Wales and with S. W. Rider in 1929 he published an interesting and useful book on the physical and economic geography of South Wales. His interest in geomorphology was maintained and in 1934 with K. L. Goskar he wrote on the coastal plateaux of South Wales; later he contributed to the Glamorgan County History.

The first paper by Trueman on the Coal Measures was published in 1923; it was concerned with the western part of the South Wales Coalfield and in particular with the mollusca in the anthracite area. This paper contains an interesting general discussion of the difficulties underlying the correlation of the Coal Measures. Trueman pointed out that any given coal seam might be traced for a few miles but for various reasons, it was rarely possible to correlate with any certainty even in neighbouring areas; yet correlation was an almost essential preliminary to the elucidation of coalfield structures; and for theoretical and practical purposes some means of identifying horizons in the Coal Measures was necessary. The identification of horizons and the correlation of seams had been mainly attempted by reference to the thickness and character of the seams and of the associated strata. Trueman stressed that the evidence derived from the fossils associated with the seams was not only reliable but permitted of more precise correlation and he suggested that such palaeontological evidence might be used with advantage in correlating seams and in determining the precise horizon of an unknown seam.

He referred to two important aspects of the palaeontological correlation of the Coal Measures, namely, to facilitate the correlation and identification of seams within one coalfield and to correlate the broad divisions or zones of one coalfield with those of another; and he again stressed not only the theoretical but also the practical value of this work, for example, in the development of new areas in the exposed coalfields and in the exploration of the concealed coalfields. He went on to review the evidence which might be obtained from a study of the fossils, both flora and fauna, and stated that the only satisfactory way of correlation would be to divide the Coal Measures into zones, each with a characteristic fauna and flora; in correlating distant areas, assemblages would not be so useful as in identifying horizons in neighbouring areas; satisfactory correlation must be based on changes of flora and fauna resulting from their evolution rather than their geographical distribution and migration; for such work accurate specific identification would be essential and since many of the fossils, both plants and animals, are species with a long vertical range, considerable preliminary work would be necessary before they could be used effectively. He considered that the only fossils from the fauna which were sufficiently abundant to be of general value were the non-marine lamellibranchs. He gave an account of the lamellibranchs in that part of the South Wales Coalfield with which he was then concerned and indicated that he proposed to extend his investigations through the whole thickness of the Coal Measures.

It had been generally thought that the species of lamellibranchs referred

to the genera, *Carbonicola*, *Anthracomya* and *Naiadites* were of little value in correlation owing to their long vertical range but Trueman pointed out that some of the specific names had been used to cover many specimens with similar shell form but not always related and forming a true species; specimens collected at one horizon but differing from one another in many ways had been illustrated under one specific name although the variations were connected by intermediate forms at that horizon; Trueman considered that it was desirable to use separate names and to regard them as members of one species-group. He stressed that the shells collected at any horizon may represent an assemblage of several species-groups, each following some particular trend in evolution and that at any horizon, the various members of a single species-group might show different degrees of advancement but the proportions of different members of that species-group are constant everywhere at that horizon. Correlation of seams in a restricted area might be possible by noting the assemblage of species-groups at each horizon; correlation of more distant areas or of other coalfields must not depend on assemblages but must be based on the stage of advancement of some one or more species-group.

This paper was in effect a statement of a programme of work on the stratigraphical palaeontology of the Coal Measures and it was quickly followed by others in collaboration with E. Dix and T. Neville George extending our knowledge of the western part of the South Wales Coalfield but in 1927 there appeared a classic paper in collaboration with J. H. Davies on a revision of the non-marine lamellibranchs in the Coal Measures of South Wales. This pioneer investigation was based upon a very careful examination of several thousands of shells of *Carbonicola*, *Anthracomya* and *Naiadites* from known horizons and it established a zonal succession which proved applicable over the South Wales Coalfield and showed promise of being applicable to other coalfields in Great Britain. It was demonstrated that in order to achieve a zonal sequence it was necessary for greater refinement in nomenclature; the use of certain specific names which had previously been employed in a wider sense was restricted and several new species were described. A statistical study was made of the shells, for example, by plotting variation in a character against the number of specimens from a horizon, and by plotting scatter-diagrams to show the degree of correlation in a series from one horizon. The study showed that the variations within each community were continuous, that each community was homogeneous and indivisible, and that such characters as for instance thickness and length of anterior end or length of anterior end and height of the shell vary independently of one another. One may see in this and subsequent work on the non-marine lamellibranchs of the Coal Measures application of the techniques which Trueman had used in his studies of Jurassic ammonites, gastropods and lamellibranchs.

There followed many other papers on the Coal Measures often written in collaboration with colleagues; with E. Dix he described the detailed succession seen in the Cwm Gorse boreholes in South Wales and attention was also given to the occurrence of important marine bands in that coalfield. The

work was extended farther afield, for example, with S. G. Clift, on the succession of the non-marine lamellibranchs in the Coal Measures of the southern part of the Yorkshire and Nottinghamshire Coalfield where again statistical methods were applied to the study of the fossils. In 1929, Trueman reviewed the progress of studies on the sequence of the lamellibranchs showing that some five zones could be recognized in several coalfields and re-affirmed his view that these fossils would afford a basis for a satisfactory classification of the Coal Measures. There followed other papers, for example, in collaboration with W. D. Ware on additions to the fauna of the South Wales Coal Measures and in 1933 Trueman put forward a correlation of the Coal Measures of England and Wales; in this latter paper he reviewed the sequence in South Wales and many English coalfields and in addition discussed the marine faunas, the floras, and the possibilities of correlation with continental coalfields. About the same time with L. R. Moore he described the stratigraphical succession in the Coal Measures of Bristol and Somerset and showed that in many respects the succession was the same as that in South Wales; some years later these authors described the structure of the Bristol and Somerset Coalfields. The results of these and other investigations were reviewed in two Presidential Addresses to the Geological Society of London published in 1947 and 1948; in the first Trueman dealt with stratigraphical problems in the Coal Measures of Europe and North America, in the second with stratigraphical problems in the coalfields of Great Britain; these addresses are an invaluable synopsis of his views about the palaeontology and stratigraphy of the Coal Measures. Meanwhile, he had commenced with J. Weir a comprehensive monograph on the Upper Carboniferous lamellibranchs which was well advanced at the time of his death. During the last years of his life, Trueman wrote parts of and edited contributions to his last book, *The coalfields of Great Britain*, an authoritative work much of which had been made possible by his investigations; it is a fitting culmination to his researches upon the Coal Measures of this country.

It will have been noted that Trueman wrote many papers with other workers and he was outstandingly successful as a collaborator in research; this was due not only to his great ability as a researcher but also to his personal qualities of friendliness and enthusiasm which inspired others interested in his special fields of study. His contacts were wide in this country and also on the continent. He visited the University of Lille in 1925 where he worked with Charles Barrois and Paul Bertrand and began a close association with Professor Pierre Pruvost now at the Sorbonne. Pruvost showed him the succession in the coalfields of the north of France and helped him to collect fossils for comparative study. He had friendly contacts with W. J. Jongmans of Heerlen, A. Renier of Brussels and many others. Trueman attended meetings of the Congress for Carboniferous Stratigraphy at Heerlen and other international gatherings where he took an active part in discussions and where his analysis of the lamellibranchs of the British Coal Measures was recognized as a basis for the comparative stratigraphy of the coal basins.

From the days when his researches were mainly concerned with Jurassic rocks and fossils, Trueman had many contacts with officers of the Geological Survey of Great Britain. These contacts were further developed as the result of his work upon the Coal Measures. He was in particularly close touch with the work of the Survey in all the British coalfields; for many years he gave professional assistance in respect of the fossils collected from the Coal Measures and he contributed to Survey publications for example in the annual reports of progress. It was therefore appropriate that in 1938 he should be appointed a member of the Geological Survey Board, which acts as an advisory body to the Geological Survey and Museum. The Chairman of the Board when Trueman joined it was Franklin Sibly and they once more found themselves closely associated in scientific and administrative work. In 1943, Trueman succeeded Sibly as Chairman and only vacated that position in 1954; but his interest remained when he was no longer able to participate directly in the work of the Board. The Board was especially fortunate to have such a distinguished geologist and such a skilled administrator during the war and in the difficult post-war years of re-organization and expansion; Trueman took a particularly active part in planning the post-war programme of work of the Survey. The writer served with him as a member of the Board for eight years and for another four years as Director of the Geological Survey and Museum; he and his two predecessors who also served under Trueman's chairmanship recognize the great services which he gave; they will be always remembered in the Survey and Museum.

Trueman was appointed Deputy Chairman of the University Grants Committee in 1946. He succeeded to the chairmanship upon the retirement of Sir Walter Moberley in 1949 and was chairman until 1953. He thus served as Deputy Chairman and Chairman in the critical years of the transition of the universities from war to peace conditions and of the great expansion after the war. Furthermore, through those years, the committee was deeply concerned to ensure that the development of the universities should be adequate for the national needs. There was thus the increasing dependence of the universities upon public funds for their income and for nearly all their capital expenditure; and at the same time the necessity for ensuring their academic independence and freedom. Trueman brought to these important tasks high academic achievement and distinction, wide experience in varied university institutions in England, Wales and Scotland, organizing ability and leadership, a clear and logical mind, and the highest integrity; these rapidly gained for him the respect and affection of his colleagues in the committee and in the universities. During the latter part of his chairmanship he had indifferent health but his courage and determination never wavered; it was sad that he fell seriously ill in 1952 and resigned a few months later.

As one of his students, Professor T. Neville George, has said, Trueman 'was at once a researcher who made outstanding advances in several geological fields, an inspiring teacher, an educationist concerned with the social implications of science, and an administrator of high achievement'.

Deservedly many honours were conferred upon him. The Geological Society of London awarded him the Murchison Fund in 1925, the Bigsby Medal in 1939, and in 1955 its highest honour, the Wollaston Medal. He received the Gold Medal of the South Wales Institute of Engineers in 1934, a particularly appropriate award since his work on the Coal Measures was commenced in South Wales. He was elected a Fellow of the Royal Society of Edinburgh in 1938 and served as a member of its Council from 1943 to 1946; and of the Royal Society of London in 1942 and served as a member of its Council from 1947 to 1949. He was an honorary LL.D. of four universities, Rhodes, Glasgow, Wales and Leeds. He was created K.B.E. in 1951.

Trueman married in 1920 Florence Kate Offler. Lady Trueman contributed greatly to his many achievements and in his last years he was sustained by her devotion and fortitude. Their son, Dr E. R. Trueman, is a member of the staff of the Department of Zoology in the University of Hull.

W. J. PUGH

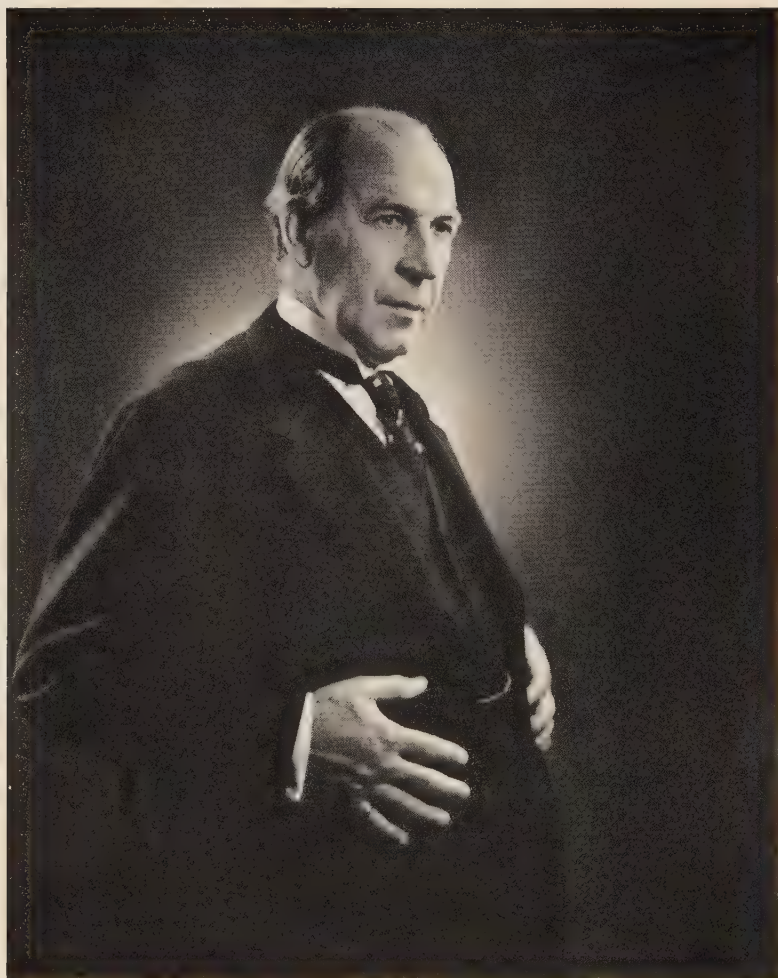
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John Underwood

Waverley.

JOHN ANDERSON, VISCOUNT WAVERLEY

1882-1958

THE RIGHT HONOURABLE SIR JOHN ANDERSON, O.M., 1st Viscount Waverley of Westdean, died in St Thomas's Hospital, London, on 4 January 1958 at the age of 75, after a lifetime of public service. Few men have filled so many public posts of the highest importance and of such bewildering variety. No one who has covered so wide a range has ever, surely, left behind a record of so many difficult tasks carried through to a successful conclusion.

But when this record of a lifetime's work is set out, it is not the variety of his achievement which leaves the deepest impression. John Anderson had certain outstanding qualities of intellect and character: and the development of these qualities and his determination to use them for the public service, give to his whole life a singular degree of unity.

Early Life and Career

John Anderson was born on 8 July 1882, the only son of D. A. P. Anderson of Westland House, Eskbank, Midlothian. He was educated at George Watson's College, Edinburgh, where he had a brilliant career and became dux medallist.

In 1899 he went to Edinburgh University, where he studied mathematics under George Chrystal and also chemistry and geology. He was the outstanding student of his day and won a host of prizes and medals. He won a scholarship in chemistry which took him to Leipzig for the 1903-4 session, where he wrote a thesis on the chemistry of uranium.

Up to this date Anderson was a scientist, looking forward probably to an academic life. But he then decided in favour of a different kind of career, more in touch with affairs, probably more rewarding and giving greater scope for his talents. He returned to Edinburgh in 1904 and read for the Civil Service examination of those days from which posts were filled in the Home Service and the Indian Civil Service. During this year in Edinburgh he took the honours course in economics. In the Civil Service examination he took first place, and gained more marks than had previously been obtained by any other candidate, with one exception.

Anderson was appointed to the Colonial Office, in which he served from 1905 until 1912 in the Nigeria and West African Departments. From the first he displayed that maturity of judgement and gravity of outlook which was characteristic of him. It was not long before his contemporaries were saying

that, when Anderson had minuted a paper, it was useless for anyone else to attempt to add anything; the Head of the Nigeria Department is recorded as having said of Anderson, while still in the junior grade of the Administrative Class, that he ought to be in his shoes as Head of the Department. During this period Anderson was Secretary of a Committee on West African Currency.

The Colonial Office, although of high standing, did not offer much opportunity in those days for rapid advancement. In 1912 he left it and was appointed to the National Health Insurance Commission, where a group of the ablest young Civil Servants from all Departments were being gathered together under Sir Robert Morant to launch the National Health Insurance Scheme.

Here at once Anderson found full scope for his abilities. He joined the office as a Principal: but a year later when the Secretaryship of the Commission fell vacant, he was appointed to it over the heads of many of his seniors. Thus, at the age of 31 he had made his way to a post of high importance, in a new and challenging department.

What impression did he make on his contemporaries? As always, they noted the maturity so remarkable in a man of his age. 'Young Jonathan' moreover soon gained the confidence of his colleagues, and those who had business to do with him often found others seeking advice from the 'friendly young Scotsman' on matters which were not his affair. And when he became Secretary, his calm clear-headed sagacity, his cool judgement, his determination to make his points, proved an excellent foil to the rather tempestuous qualities of Sir Robert Morant, the Chairman of the Commission.

The Insurance Commission itself consisted for the most part of persons whose experience had lain outside Whitehall and whose approach to problems was not always very acceptable to Whitehall. Anderson's personality and perseverance provided just what was needed to convince the Commission that there was a lot to be said for the orderly ways of working inevitable in Whitehall. But if any member of the Commission threatened to kick over the traces, he had a way of looking out over the top of his eyeglasses, which was mightily discouraging.

When war broke out in 1914, Anderson remained at first at the Insurance Commission, but was given two extra responsibilities. The first was to find some means of providing the nation with the pharmaceuticals for which we had previously been wholly dependent on German sources of supply. The second was to redistribute, on a voluntary basis, medical practitioners so as to meet the needs of industrial areas, and in particular the areas devoted to war industries, whose medical personnel had been dangerously depleted through the demands of the Army.

These two responsibilities were typical of the special assignments which Anderson was asked to undertake by the dozen throughout his life: no attempt has been made to list them all in this note. They called for the capacity to get hold of the basic facts quickly, to perceive the essence of the problem

and how it could be solved, to devise effective machinery to achieve the result desired, and above all to create confidence in the interested parties that their affairs were being handled in a fair-minded objective way which compelled their co-operation. All these qualities Anderson possessed in abundance.

In 1917 when a Ministry of Shipping was set up to take over from the Admiralty the handling of merchant shipping, Anderson was released from the Insurance Commission to become the Secretary of the new Ministry. He had no knowledge of shipping, but soon won the confidence of Sir Joseph Maclay (later Lord Maclay) and his council of shipowners, and did much to ensure that the work of the Ministry was carried out in an orderly, effective way. The scheme, developed in the later stages of the war, of Shipping Programme Committees (at that time rather a novel idea) headed by an Allied Maritime Transport Commission and Executive, owed its initiative to others, but Anderson's influence had much to do with its adoption.

At the end of the war Anderson was appointed to four important posts in quick succession. In April 1919 he was made an additional Secretary to the old Local Government Board. Later in 1919, when the Local Government Board and the Insurance Commission were merged in the new Ministry of Health, he became Second Secretary of that Ministry under Morant, who had strongly pressed for his appointment.

In October 1919 he was appointed Chairman of the Board of Inland Revenue. He remained formally Chairman until 1922. But in 1920 fate had other things in store for him.

Ireland

About this time affairs in Ireland were becoming desperate and it was evident that a strong pair of hands was needed. Sir Warren Fisher, then recently appointed to be Permanent Secretary to the Treasury, recommended Anderson's appointment to Ireland for two reasons. The first was that he had *par excellence* the qualities needed for the post. The second was that Anderson's first wife, whom he had married in 1907, died very suddenly in 1920 at the age of 34; and it was felt that a change of scene, and a job which called for all that even Anderson had to give, would be welcome to him.

In Ireland Anderson was given a dual role. Many felt that the Treasury had been too reluctant to sanction military expenditure in Ireland. To get over this difficulty, Anderson was made Treasury Representative in Ireland. He was directly responsible to the Chancellor of the Exchequer, from whom he received a wide delegation of powers, and a special staff was seconded from the Treasury to work under him in Dublin Castle.

He was also put in charge of the political side under Sir Hamar Greenwood, M.P., the Chief Secretary. (Nominally Anderson was Joint Secretary with Sir James MacMahon, but the latter played little part in affairs.) A new

staff was imported from England to infuse life and vigour into the direction of affairs.

The Government's policy in Ireland can be described, broadly, as conciliation combined with coercion, a policy difficult to carry out at the best of times. After two intensely difficult and troublesome years, Anderson's period of service in Ireland ended with the establishment of contact with the Sinn Féin leaders, who were persuaded to negotiate with the British Government. The conduct of these negotiations was in the hands of Mr (later Sir) Alfred Cope, but were carried out with Anderson's full knowledge and authority. In this, as in other matters, he gave a free hand to his subordinates while ready to help with advice and guidance at all times: and he always gave his staff the fullest support.

These two years were important in Anderson's career. They imposed on him a burden of work, and a degree of responsibility in a country rent by civil war, which almost any other man would have found intolerable. Added to this were the physical dangers to which he was constantly exposed. One gains the impression that these years of strain provided a further toughening of his character, a further sharpening of his keen brain and a widening of his experience—in one who was already exceptionally well endowed in all these respects.

The 'Young Jonathan' of a few years earlier now gives place to the kindly yet rather formidable and at times aloof figure which Whitehall was to know so well later on.

Here is a contemporary description of the impression which Anderson left on one who saw him at close quarters in those years in Dublin Castle:

'Deep-set inscrutable eyes, barely visible while in thought, formed a mask to the whole countenance. Open, they looked at you with something of reserve, something of humour, touching up the impassiveness of the long dark face. In official life his manner had the touch of greatness in it—easy, gracious, but with something of condescension, as of one born to rule. Deliberate, yet swift in judgement, without superfluity, grasping the pith of the matter in a moment, he disclosed that remarkable versatility which enabled him to hold his own with men who had made a life-study of what he had first heard of but yesterday.'*

Home Office

On his return from Ireland Anderson was appointed Permanent Under-Secretary of State at the Home Office and served in that capacity from 1922 until 1932, his longest spell in any official post. One of his earliest tasks in his new post was to deal with questions relating to the establishment of the Government of Northern Ireland. The confidence which Sir James Craig (later Lord Craigavon) had in Anderson was a great help in fostering the

* 'The last Days of Dublin Castle', an article in *Blackwood's Magazine*, no. MCCLXXXII, for August 1922.

friendly relations between the Northern Ireland Government and the United Kingdom Government.

Anderson also had a great influence on the improvement of police work. In 1922 a new policy had been instituted (of which Sir Arthur Dixon was perhaps the chief architect) to make the Home Office an effective centre of information and guidance in police work. Anderson put the full weight of his authority behind this policy. The establishment of good relations between the Department and the local police authorities and their Chief Constables was mainly due to the deep impression which he made at all conferences and meetings concerned with police administration.

So too with prisons. Perhaps the most striking figure in the Prison Service at this time was Alexander Paterson, a practical idealist, but the antithesis of an administrator, whose thoughts and actions were all based on spiritual considerations. Anderson fully understood Paterson and recognized his fine qualities, Paterson, on his side, had complete confidence in Anderson's judgement. The combination of two such different personalities did much to help forward the reform of prison administration.

During the General Strike of 1926 there were many examples of the way in which Anderson's immense capacity and force of character made him a natural seat of authority. As Chairman of innumerable committees, he guided, co-ordinated, and regulated the activities not only of many Government committees, but also of other bodies such as police authorities, dock and transport authorities and commercial associations. The representatives of these bodies readily came to any meetings which he arranged because they welcomed the opportunity of telling him their difficulties and getting his advice.

During these years Anderson was, of course, one of the foremost figures in Whitehall and was called upon to lend a hand in many matters outside the purview of the Home Office. Thus he was Chairman of a Committee of Permanent Heads of Departments, which prepared three reports to the Cabinet on the extended scheme of social insurance. These reports were the basis of the Widows, Orphans and Old Age Contributory Pensions Act 1925.

India

In 1931 once again Anderson was asked to undertake a post where a strong hand and cool judgement were needed to deal with a difficult and dangerous situation. He was appointed Governor of Bengal and assumed office in February 1932.

He had many difficulties to overcome. The first was terrorism. There had been a series of terrorist outrages shortly before his arrival. The Inspector General of Prisons and others had been killed in the Secretariat, there had been the Chittagong Armoury raid, and three officers in succession had been killed at Midnapore. There had also been an attempt to assassinate his predecessor (Sir Stanley Jackson) shortly before Anderson's arrival. The

morale of the Services had been considerably impaired by these events, but with Anderson's arrival confidence was soon restored. This was due, in part, to the improved security measures which he introduced, but also to the deep impression made by his own personal qualities—his imperturbability and contempt of danger.

But Anderson was not content merely with prevention. He saw the need for remedial action and the removal of underlying causes; and he set himself from the beginning to the rehabilitation, the welfare, and the re-education of the misguided young men who had been caught up in the terrorist movement. He was responsible for the establishment of vocational training centres for *détenus*, for securing financial assistance for men so detained to establish themselves in small industry or in business, he interested himself actively in individual cases, and he ensured periodical and thorough reviews of all sentences of detention.

In 1934 an attempt was made to assassinate him. He was shot at by a young man on the Lebong racecourse, at short range and had a miraculous escape. This attempt at assassination proved indeed to be something of a turning point in the campaign against terrorism; for popular opinion in Bengal as a whole was so deeply shocked by this attempt on his life that the anti-terrorist forces gained considerably in strength. Indeed, by the conclusion of his term, the terrorist movement had been brought under close control.

It was typical of Anderson that as a result of his personal intervention the youth who shot at him in 1934 was released from detention and sent to the United Kingdom for training and that Anderson had many letters from him. This act produced a swift and sympathetic response from the people of Bengal.

He had also to deal with a serious economic situation in the Presidency. There was a running deficit estimated at 2 crores of rupees (say £1½m.). Over the period of his administration he turned the deficit of 2 crores into a surplus of slightly under half a crore. He succeeded in obtaining from the Government of India, consequent on the report of the Niemeyer Committee, the transfer to provincial revenues of the jute export duty, and the complete remission of the financial liabilities of the Presidency to the Central Government, and he added from various other sources at least a crore of rupees to the annual revenues of the Province.

In other areas of the economic field his contribution was great. He was responsible for a campaign, in its later stages largely operated by voluntary helpers, against the menace of the water hyacinth, which threatened to clog the bulk of the waterways of Bengal; he successfully brought about a regulation of jute production through voluntary restriction; not long after his appointment he established a panel of economic enquiry to tackle the problem of the indebtedness of the rural population, and he was behind an Agricultural Debt Settlement Act for scaling down the accumulated debts of the agriculturists.

Finally, it was under Anderson that Bengal entered on the enjoyment of provincial autonomy under the provisions of the Government of India Act of 1935. There could have been no more skilful pilot at the helm. His great experience of government in the United Kingdom, his clear-headedness, and the reputation which he had earned in the Presidency during his preceding five years as Governor as a sympathetic administrator, devoted to the interests of Bengal, all made him the ideal leader for this new experiment. When he handed over to Lord Brabourne, six months after the introduction of provincial autonomy, the new system was well on its way.

Cabinet Minister

Anderson's original term of appointment of five years was extended for another six months and he returned to England in November 1937. While on his way home by sea, he was invited to be a candidate for the Scottish Universities seat in Parliament which had become vacant through the death of Mr Ramsay MacDonald. He stood as a National candidate and was elected in February 1938, retaining the seat until University representation was abolished with effect from the Dissolution of 1950.

Anderson also became a Director of the Imperial Chemical Industries and of the Midland Bank. But within a year of his return to England he was once more in the public service. After the Munich crisis various steps were taken to intensify our defence preparations. One of the most important was the appointment of Anderson as Lord Privy Seal, with a seat in the Cabinet and responsibility for Civil Defence. This was no easy task. Hitherto Civil Defence had been regarded as chiefly the concern of a sub-department of the Home Office. Anderson saw that any effective Civil Defence policy called for active participation by most of the civil agencies of Government, and he set himself to devise an organization in which dispersal of functions was combined with effective central control.

Two of the many measures which he set in hand are worth mentioning. The first concerned shelter policy. He asked Mr (later Sir William) Paterson to devise a shelter capable of being made in large numbers at small cost and put up in any back garden. Paterson's design was accepted with but few modifications. It became a household word as the 'Anderson shelter'. The second was his appointment of the Civil Defence Research Committee. The work done under the auspices of this body on the physics of explosions and the effect of explosions on buildings, later proved to be of value for offensive as well as defensive purposes.

When war broke out in September 1939 it was no longer appropriate for Civil Defence to be the responsibility of a non-departmental Minister: and Anderson was appointed Home Secretary and Minister for Civil Defence, thus returning to the Department of which he had been the Permanent Secretary for ten years. At the outset of the war Anderson was not made a

member of the War Cabinet but he was one of the Ministers who were in 'constant attendance'.

In October 1940, when Mr Neville Chamberlain resigned from the Government on the grounds of ill-health, Anderson was appointed to succeed him as Lord President with a seat in the War Cabinet. The work which he did in this post over the ensuing three years was perhaps the greatest of Anderson's many public services. It was certainly of supreme importance for the war effort.

The War Cabinet system had been found by experience to work most effectively when many matters, of sufficient importance to be referred to the Cabinet in a peace-time regime, could be thrashed out in a subordinate body where either a decision would be reached or a recommendation framed in a form which required no long consideration by the War Cabinet itself. So far as concerns the military conduct of the war, such a body existed in the Defence Committee, which as time went on, played an ever-increasing part in the direction of military matters under the Prime Minister's leadership. A corresponding body was required on the civil side, and, as Lord President, Anderson was in charge of a Ministerial Committee which did this very necessary work.

The underlying purpose of Anderson's work as Lord President was no less than to make sure that the civil side of the national effort was carried out in a way which would give most help in winning the war. This involved a number of what in the United States might be called 'overall studies'. Here he was helped by having at his disposal the services of the Central Staffs of Economists and Statisticians recently set up in the Cabinet Secretariat. But much of his work had an intensive practical day-to-day application, which bit deep into departmental activities and indeed into the life of the nation. Long before the end of the war, the chief instrument in apportioning the resources of the country had proved to be the allocation of manpower (and womanpower) to the various sectors of the economic life of the country. It was under his direct guidance that the material required for this apportionment was collected and appraised, and it was his recommendations which went forward to the War Cabinet.

The work of the Lord President also involved a great volume of work where wide differences of view arose between Ministers and Departments. Here all Anderson's qualities made him ideally suited for the role he was called upon to play: above all his quickness in seizing hold of the essentials of the situation, his clarity in exposition, and the outstanding fair-mindedness of his approach.

Two other factors helped to make Anderson readily acceptable as the resolver of differences between other Ministers. One was that he worked as far as possible through departmental staffs and did not build up any large organization of his own. His decisions were patently the result of his own consideration and judgement. The second is perhaps harder to describe. The essence of it was that Anderson had the gift of disengaging at the earliest

possible moment when he had marked out the lines on which a particular matter should be settled, and of leaving the Ministers concerned to carry on from that point without his intervention. Here he was powerfully aided by the lack of any desire on his part to take any leading role in political affairs.

Anderson's work as Lord President could not have been done without complete confidence between the Prime Minister and himself. In temperament and in their methods of work the differences between the two men were obvious enough. But their strongest qualities were complementary: and it is clear that each recognized this to the full, and that the effectiveness of the partnership owed much to this mutual recognition.

(While Lord President, Anderson had much to do with scientific matters. This is dealt with on pages 318-20.)

During the period that Anderson was Lord President, the post of Viceroy of India fell vacant and there was talk of sending him to India as Viceroy. Earl Attlee has recalled how Marshal Smuts came up to him and said: 'Don't let them do it. In every War Cabinet you need one person who looks after the machine. Milner did it in our first Government, and John Anderson is doing it in our second.'* In the event this argument won the day.

In September 1943 Sir Kingsley Wood, Chancellor of the Exchequer, died suddenly and Anderson was appointed to succeed him. He held the post until the General Election of 1945.

The role of Chancellor of the Exchequer in wartime has to take its place in the national war effort. Much of Anderson's time was taken up with financial relations with our Allies and with overseas countries and, at the end of his period of office, with our defeated enemies. He also put through the P.A.Y.E. scheme which his predecessor had brought to the stage of a White Paper and which involved two Acts of Parliament. In the Finance Act of 1944 and the Income Tax Act of 1945 he also gave tax relief to expenditure on scientific research: in 1944 for revenue expenditure, and in 1945 for capital expenditure, as part of a wider scheme.

Again, as Lord President a good deal of the responsibility for general co-ordination of the country's economic policy had rested on Anderson's shoulders. When he became Chancellor of the Exchequer, although there was no formal re-allocation of duties, much of this responsibility went with him. There was also a tendency for him to be looked to, by the Prime Minister and other Ministers, for help when serious trouble was brewing. Besides all this, he had his work on 'Tube Alloys' (see page 319). The burden of work on him was therefore very great indeed, but he carried it without any loss of resilience.

The value of Anderson's work as a War Cabinet Minister, and the very high regard in which he was held is shown by the following.

In his official biography of King George VI, Mr J. W. Wheeler-Bennett records that in 1942 the King asked the Prime Minister whom he would recommend should be asked to succeed him in the event of his death as a

* *House of Lords debates*, 28 January 1958, Vol. 207, Number 26, Column 221.

result of enemy action. Mr Churchill (as he then was) formally tendered advice, before leaving for Washington in June, 1942, to visit President Roosevelt, that in the event of his death His Majesty should entrust the formation of a new Government to Mr Anthony Eden.

Early in 1945, both Mr Churchill and Mr Eden were about to be absent simultaneously in order to attend the Yalta Conference; and again King George sought advice as to who should be asked to become Prime Minister if both Mr Churchill and Mr Eden should be killed on this forthcoming journey. In a long letter of 28 January 1945, Mr Churchill recommended that in this contingency His Majesty should send for Sir John Anderson. The letter explains at some length that this advice was given more particularly in relation to the period 'when the war against Germany hangs in the balance and when a National Coalition Government is functioning. . . . It would therefore seem very appropriate that during the continuance of the Coalition a Prime Minister should be chosen who would not necessarily be the Leader of the Conservative Party, though that might well follow, but would be adapted by character and outlook, and by the general regard attaching to him well qualified to sustain the existing all-Party Government.'*

1945-1958

Anderson never held office again after the defeat of the Churchill Government in the election of 1945. In the ensuing years the Opposition Leaders kept in touch with him, and he attended meetings with them on several occasions, when his views were sought on matters of great importance. The basis of the relationship seems to have been one which permitted the Opposition to hope that Anderson would once more take office, when a Conservative Government was again returned to power, but which was also consistent with Anderson's firm determination not to become a member of any political party. After the abolition of University representation, more than one opportunity was offered to him of being Conservative candidate for a safe Conservative seat. But to all such offers he gave the same answer. As a Civil Servant, he had served Ministers of both the main political parties and had been their trusted and confidential adviser, and he did not feel that it would be right for him to sit in Parliament as a member of either party. It was thus by his own decision, and by his steadfast refusal to abandon a position independent of the main political parties, that his career in Parliament was not resumed. This was no small sacrifice on his part.

In 1952 he was raised to the Peerage as Viscount Waverley of Westdean in the County of Sussex. But as this note does not follow a strict chronological sequence, it is convenient to continue to speak of him as Anderson.

The years from 1945 until his death in 1958 were full years, given to much public service of the most varied kind.

* *King George VI, his life and Reign*, pp. 543-6, by J. W. Wheeler-Bennett, C.M.G., to whom I am greatly indebted for permission to use the information on which this passage is based.

Anderson's main task in these years was the Chairmanship of the Port of London Authority, to which he was appointed in 1946 and which he held at his death. This work meant a great deal to him. From the outset he saw the closeness of the link between the Port of London and the City of London. Indeed, he believed that the greatness of the City depended in no small part on London continuing to be a great port. He set himself deliberately, by methods which included much gracious hospitality, to ensure that the leaders of the City and of the nation as a whole comprehended the work of the Port and its importance. As a result, the prestige and high standing of the Authority were raised, both nationally and internationally. He enjoyed to the full his contacts with the world of shipping and commerce, and with the Corporation of London. He had, too, a very high standard of how a Public Authority should conduct its affairs, both in its dealings with the general public and in its relations with other public authorities. He insisted that these relations must be carried on on the basis of friendly discussions and mutual respect for each other's interests.

Anderson put at the service of the Authority his scientific as well as his administrative gifts. On his initiative, a series of experiments was set on foot, using the latest scientific devices, aimed at securing, for the first time, some exact knowledge of the tidal movements of the Thames and of the forces which result in the deposit of silt in particular reaches.

Among his other directorships he was also a Governor of the Hudson's Bay Company and it is believed that he had a hand in devising the title 'Grand Seigneur' conferred by the Company on Sir Winston Churchill. By virtue of his association with the Hudson's Bay Company and the Canadian Pacific Railway, he and Lady Anderson visited Canada every other year for many years. They travelled all over Canada several times, to the far north and the north-west and to the west coast. Wherever they stayed they met the representatives of the Canadian Pacific Railway and the Hudson's Bay Company and many other people. As a result of these travels, it was said that they had visited more places in Canada than most Canadians; and he also got to know many prominent Canadians, who never failed to visit him when they came to this country. Canada meant a great deal to him: and when the time came that he could leave London in August and September, his first wish was to visit Canada.

He also paid a visit in 1950-51 to Australia in connexion with the business of the Port of London Authority, and to New Zealand as a guest of the New Zealand Government. This visit he had hoped to repeat.

Anderson was a keen botanist and was accustomed to identify at sight any flower that he saw. But in Australia he found flowers and plants unknown to him. He was also excited by the geology of the country and took a deep interest in the growth of its new industries.

Among his other directorships should be mentioned that of the Imperial Chemical Industries, where his judgement was much valued alike as a statesman and as an administrator, and for his scientific background.

In 1946 he succeeded Lord Keynes as Chairman of the Covent Garden Opera Trust. He disclaimed any special knowledge of the arts. His role as Chairman, as he saw it, was to ensure that the administration was carried out on sound lines, and to use his influence to support the claims of the Trust in difficult times. He and Lady Anderson were constant in their attendance at the Opera House, and frequently acted as host and hostess. His constant and unflagging support of the Trust, his steadfastness in moments of crisis, and his constructive work in council, were of the greatest value.

He continued to be in constant demand as chairman of Government committees. One of the most important of these was the Waverley Committee on the Export of Works of Art. This dealt with the problem of works of art which their owners wished to export, but which the museum authorities thought should not be allowed to leave the country. The recommendations made in this report were, broadly speaking, adopted and govern the present practice.

He was also Chairman of the Commission appointed after the disastrous floods of January 1953, to enquire into our sea defences and warning systems.

One important chairmanship, for which he was specially suitable, he was not able to carry out. In 1952, shortly after he had been raised to the Peerage, he was appointed as Chairman of the Royal Commission on Taxation of Profits and Incomes in succession to Lord Cohen who had been made a Lord of Appeal in Ordinary. This led to protests from the Opposition benches, and the Prime Minister was asked by a leading member of the Opposition whether he realized that in the 1945-50 Parliament Lord Waverley had shown that he held strong political views corresponding to those of the Conservative Party. When the Opposition decided to raise the matter in debate, Lord Waverley wrote to the Prime Minister resigning his appointment. In his letter, published in *The Times* on 1 March 1952, he referred to 'unjustified personal aspersions', and said that the attacks were the more wounding because they appeared to have the acquiescence if not the active support of wartime colleagues whom he regarded as his personal friends. No doubt he felt hurt that the Opposition did not accept that he had 'always been regarded as a personality above the turmoil of party politics', to quote a phrase used in a letter to him from the Prime Minister, published in *The Times* on the same date.

He was also President of the Royal Institute of Public Administration from 1944 until his death. Far from treating this as a wholly honorific office, he made a point of attending many of the Institute's functions and lectures and was always ready to give his support and counsel.

Contribution to Science

As Lord President from 1940 to 1943 he was the Minister in charge of the Civil Scientific Departments: the Department of Scientific and Industrial Research, the Medical Research Council and the Agricultural Research

Council. More important than his normal responsibilities in connexion with these three bodies were certain general developments which took place during his period of office. It was in this period that the application of science to industry came to be much more widely recognized as a necessity. Prompted by him, Sir Edward Appleton, Secretary of the Department of Scientific and Industrial Research, 'stumped' the country (his own phrase) using the scientific successes in war as an example of what might be done in peace.

Again, scientific opinion in the country felt that the methods of bringing scientific views and ideas to the notice of the Government were unsatisfactory and worked too slowly. Anderson took a hand in setting up a Scientific Advisory Committee with the widest possible terms of reference, to act as a speedy and more effective channel of communication. The first Chairman was Lord Hankey, who was succeeded by Sir Henry Dale when the former left the Government in 1942. This piece of organization (now the Advisory Council for Scientific Policy) has proved of permanent value.

Anderson's biggest contribution to the advancement of science was in respect of nuclear energy. In August 1941 Lord Cherwell reported to the Prime Minister that so much progress had been made with research into the release of energy by nuclear fission that there was a reasonable chance that an atomic bomb could be produced before the end of the war. It was decided that a Cabinet Minister should be made responsible for this work and that Anderson should be the Minister concerned.

He therefore assumed control of the work subject only to reference to the Prime Minister. A special section was set up in the Department of Scientific and Industrial Research to handle the work under Mr (later Sir Wallace) Akers. The work was treated throughout with the greatest secrecy and was given the code name of 'Tube Alloys'.

Anderson had the help of an Advisory Committee. Nevertheless, a great deal of personal responsibility fell upon him.

Thus after the German invasion of Denmark, it was Anderson's decision which led to a Mosquito aircraft being sent to rescue the celebrated Danish scientist Nils Bohr and his son and to bring them to England, where they were appointed to the British team. Bohr remained a member of the British team to the end, despite representations from the United States.

In what might be called the 'scientific administration' of atomic energy, his was the final responsibility for deciding which of the lines of work suggested should be followed up, and he was responsible for obtaining finance to carry out the policy decided upon. He also had to carry out a great deal of what was really diplomatic work and had many discussions with the U.S. Ambassador. At a later stage he was involved in negotiations with other Governments about raw materials.

He visited the U.S. on atomic business to carry out the negotiations which led to the Quebec Agreement. This Agreement made it possible for collaboration between the U.K., U.S., and Canada to be resumed after the interruption in 1942 when the U.S. project came under Army control as the

'Manhattan Project'. Soon after the Agreement was made, British teams went to work in U.S. on the bomb and the Chalk River project was founded as a joint Canadian-U.K. project. It is indeed difficult to say how all this extremely important development could have been carried out so successfully under stress of war had there not been a Minister of the Crown with his very special qualifications for the work.

When he became Chancellor of the Exchequer he continued, at the Prime Minister's request, to carry the responsibility for 'Tube Alloys'.

When the Labour Government had taken up office after the Election of 1945 he was asked to continue as Chairman of the Advisory Committee on Atomic Energy working under the new Prime Minister. He accepted and continued to do this work until January 1948. He accompanied Mr Attlee (as he then was) on the visit which he paid to North America to see President Truman and Mr Mackenzie King in 1945, when the matters discussed included atomic energy.

Later still he was Chairman of the Committee appointed by the Government to devise a plan for transferring responsibility for atomic energy from the Ministry of Supply to a non-departmental organization. The organization of the present Atomic Energy Authority closely follows the recommendations of this Committee. His connexion with atomic energy did not cease then. He was always available for consultation and his wise counsel was of inestimable help in solving the difficult problems inherent in the transition to the new organization.

Science also owes much to Anderson for the work he did in securing a better recognition of the place of science in the life of the nation and better financial provision for its needs.

In 1945 he was elected a Fellow of the Royal Society under Statute 12 which covers those who have 'rendered conspicuous service to the cause of science, and whose election would be of signal benefit to the Society'. It is difficult to think of anyone to whom these words could more aptly apply.

Personal Qualities

At the beginning of this note it was suggested that, despite the wide range and variety of his achievements, Anderson's life had a singular degree of unity, and that the more one considers his diverse achievements, the more one sees running through them all, the intense and single-minded use for the public good of a most remarkable range of qualities.

Take first his devotion to the public service. Of the period of fifty years between the date when he entered the Civil Service and his death, a half (twenty-five years) was spent in the Home Civil Service, of which half was spent as Permanent Secretary to Government Departments.

Another quarter was spent as Governor of Bengal, or as a Minister of the Crown. During the remaining twelve years, he was for five years a Member of

Parliament and throughout the whole period carried out a great variety of public work.

Anderson never forgot that he had been a Civil Servant. He cared deeply for the well-being and impartiality of the Service and for the maintenance of its high standards. It was this sense of belonging to the public service that, nearly twenty years after he had left the Home Civil Service, made him refuse the offer of a Conservative seat. And when on his deathbed he was told that the Queen intended to confer upon him the Order of Merit, his first remark was: 'the Civil Service will be pleased about this.'

Many others have described his qualities, but this note would not be complete without some account of them.

First one puts his vast and widely assorted store of knowledge. The extent of this knowledge would have been remarkable in any man. It was amazing in a man of affairs who did not seem to spend much time in reading. Moreover his knowledge seemed to improve with keeping. Perhaps this was because everything which he heard or read seemed to attach itself automatically to what he had learned before on the same subject, so that his knowledge of it was continuously enriched.

Perhaps too this was why, although he did not mind if one could not answer a question, he could be seriously annoyed at being given false information, which of course would have taken a place in his permanent accumulation of knowledge and might have contaminated it.

But it did not really matter to Anderson whether or not he started with any prior knowledge of a subject which he was called upon to handle. In a very short time he could attain a mastery of it sufficient for the purpose in hand. For example, when he was appointed Chairman of the Committee on the Export of Works of Art, he learnt enough within a few weeks to be able to carry out the main examination of the expert witnesses without once falling into error.

Next comes his capacity to analyze a problem into its component parts. Having done this there followed a rapid judgement as to what needed to be done—a judgement which seemed rarely, if ever, to be at fault. Finally there came his power to give a clear exposition of the whole—every part of the exposition falling into exactly the right place and the whole being infused with a sense of almost judicial objectivity.

Everyone has his own story of how Anderson showed these qualities on some occasion. Nobody (the writer included) is ever happy until he had told his own story.

In about 1942 certain changes were proposed in the organization of the Central Economic and Statistical Staffs in the Cabinet Office. Being ignorant of this matter, but rather distrustful of what was proposed, I sought out Anderson without disclosing what I wanted to consult him about. I told him of my anxieties. Yes, he said, it was an interesting, a difficult question. There was a good deal of diversity of practice. In Canada they had such and such an organization. In the U.S.A. something different. The Australian

organization was touched on. He proceeded to speak of the position in the chief European countries. And then, warming to his subject, as still further supplies of knowledge flowed in from the outer recesses of his mind; yes, it seemed clear that there were two contrasting schools of thought on this matter, which he proceeded to set out. Both, as I would see, had respectable support. On the whole, he thought that the one about which I had expressed doubts had, on balance, the advantage. And I went away, knowing that I had obtained more information and got better advice in a quarter of an hour than if I had spent weeks in consulting a dozen other men.

In his handling of men, Anderson practised the greatest possible degree of delegation. He expected to be kept informed of what was happening; to be asked for advice on difficult questions; and to be brought into action when a crisis supervened. But he was always ready to leave the man in charge to carry on with his job. And he was always resolute in supporting his subordinates in the action taken. He showed the same quality in dealing with drafts submitted to him and always contented himself with the fewest possible alterations.

He expected good work from others and he got it. He gave praise readily where it was deserved, and any outstanding piece of work, or anything which had called for a special effort, always received recognition. But those whose work fell short of what he wanted because they had not got it in them to do what was required, were treated with amazing tolerance. To himself he was unsparing. The public good came before anything else. He had no fear and was never deflected from doing what he knew to be right. But he often showed a surprising degree of understanding of the point of view of those with whom he found himself in conflict.

It was the exercise of these qualities in so many fields that made him a supremely good administrator, without doubt the outstanding administrator of his age.

Outside the field of pure administration, when politics came into the picture, he did not always show the same mastery. In the House of Commons he was trusted, liked and respected, but he did not catch the imagination of the House in the way that might have been expected. Perhaps he was too ready to include in his speeches the fullest detail and lost thereby some of the sympathy of his audience.

But the nickname 'Jehovah' was, as has been well said, 'a kindly title . . . and a recognition . . . even in jest that here was someone built on lines distinctly grander in scale than those of everyday men'.*

On social occasions, particularly in his earlier years, he was often alarmingly devoid of small talk. This gave him the reputation of being ponderous and lacking in human sympathy. That he gave this impression on occasion is not to be denied. But it did not represent his nature. He had a full measure of human sympathy and understanding: he often showed the strongest indignation at the plight of some person who had been ill-treated. But in any

* Article in *The Scotsman* of 6 January, 1958, by the late Rt Hon. Walter Elliot, M.P.

public speech or statement a certain innate seriousness of purpose, and perhaps shyness, led him invariably to follow the intellectual line of approach. He was apt to speak as though the emotional and human side of things was not something separate from the intellectual, but something comprehended in it. And this at times gave an impression of stiffness.

In later years, and particularly after his second marriage, his temperament mellowed. His stiffness and shyness gave place to the unforced geniality of a good host and of a man on good terms with himself and others, and with ready sympathy for younger men.

Throughout his life, all those who worked closely with him came to regard him not only with respect, but with strong liking and affection.

Anderson received many honours. He was made a Privy Councillor of Ireland in 1920 and of the United Kingdom in 1938; a G.C.B. in 1923; G.C.I.E. in 1932; G.C.S.I. in 1937; a Viscount in the 1952 New Year's Honours and he received the O.M. in December 1957.

He also received many honours from foreign countries. He was a Grand Officer of the Legion of Honour; Commander of the Crown of Italy; a Knight Grand Cross of the Order of St Olaf, of the Royal Order of North Star, of the Military Order of Christ, and of the Order of Dannebrog. He also received the Order of St Anne.

From Edinburgh he received the degree of LL.D. in 1938, the Freedom of the City, and the Honorary Membership of the Edinburgh Merchant Company.

He also had many other academic honours. He was an Honorary D.C.L. of Oxford, an Honorary D.Sc. of McGill and Reading, an Honorary LL.D. of Aberdeen, Cambridge, St Andrews, Liverpool, Leeds, Sheffield and London. He was an Honorary Fellow of Gonville and Caius College, Cambridge, and he delivered the Romanes Lecture at Oxford and the Stamp Memorial Lecture at London University.

He was an elder of the Church of Scotland (St Andrew's, Calcutta).

He married first in 1907, Christina, daughter of the late Andrew Mackenzie, by whom he had one son and one daughter. She died in 1920. In 1941 he married Ava, daughter of the late J. E. C. Bodley, and widow of Ralph Wigram, C.M.G.

This note could not have been written without the help of many friends and colleagues of Lord Waverley, who have worked with him at periods of his life over the past fifty years, but my greatest gratitude is to Lady Waverley.

BRIDGES

ADDENDUM

BY SIR HENRY DALE, F.R.S.

Lord Bridges has asked me to add, to his Biographical Memoir, a note on certain matters of my own experience, concerning the outstanding services rendered by Lord Waverley, at different periods of his great career, to the interests of science, and their relation to those of the nation.

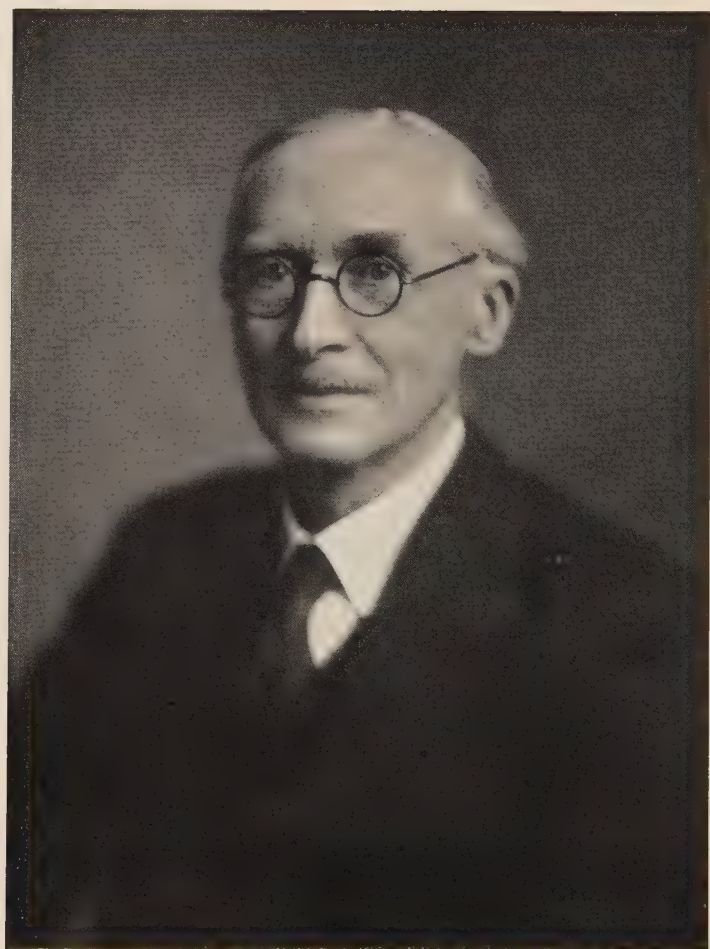
I first met John Anderson soon after the outbreak of the First World War. He was then a young civil servant with an already brilliant reputation, and with the unusual background of an earlier and direct experience of chemical research. Sir Robert Morant had found himself, on behalf of the new National Health Service, suddenly faced with the urgent problem of maintaining the supplies of important remedies, and, in particular, of replacing from other sources, and, if possible, by emergency production in this country, the many for which British medicine had then come to be wholly dependent on Germany. John Anderson clearly had special qualifications for such a responsibility, and was soon busy with it. I had just been appointed to the research staff of the then new Medical Research Committee (now Council), and had been given, on their behalf, certain analogous duties in relation to other services; so that it was natural and necessary that we should meet and work together, as we did on many matters of common concern. And, in the same connexions, we served together then on special Committees which the Royal Society had appointed, to organize and direct into nationally important channels such ability in chemical research as had not already been absorbed by Government establishments.

I lost touch with John Anderson, except for casual meetings on public occasions, in the period between the great wars, during his meteoric rise in the Service, his Governorship of Bengal, his eventual return and his historic term of office as Home Secretary, with its multiple responsibilities for civil defence precautions. It was later in the second war, when he had become Lord President of the Council, and I was President of the Royal Society, and then Chairman of the confidential Scientific Advisory Committee to the War Cabinet, that, after many years, I was again brought by war necessities into close association with Sir John Anderson. He was then the Minister charged with Cabinet responsibility for all the top-secret researches, developments and inter-allied dealings, concerned with atomic energy and its belligerent possibilities; and he required, of course, a very small and intimate scientific committee to assist him in discharging that responsibility. And, with regard to his services to science in that connexion, and in others as well during those fateful years, I do not think that it would be possible to exaggerate the importance to the national interest, of having in such a position of authority, behind the dense curtain of official secrecy, a man with so natural an understanding of the meaning of science, and so instinctive a knowledge of, and sympathy for, the possibilities and the difficulties of research. And it was, of course, a quite remarkable piece of national good

fortune, that the man charged with that responsibility should, many years earlier, have received the Ph.D. at Leipzig for his thesis on the chemistry of uranium.

There were other and quite different connexions in which Lord Waverley had shown the breadth, as well as the deep sincerity, of his interests in the promotion of science. For years, for example, he devoted his experience and his wise judgement to the steady service of the Lister Institute, as the Treasurer of its Governing Body. It was he, indeed, who, towards the end of the war, brought pressure to bear on me with regard to its then vacant Chairmanship.

The Royal Society is justly proud of the roll of the distinguished statesmen and public men, who, over the centuries, have accepted election to its Fellowship. It seems to me unlikely, however, that for any other of them so strong a claim could have been made, as now for Lord Waverley, on the ground of direct and devoted service to the Society's special purpose—the advancement of natural knowledge. And, from personal observation, I am convinced that none of them would have given a higher place to our Fellowship in his esteem, among all the other honours which he had received. For a moment there was a suggestion, very rare with him, of a deeply emotional reaction, when Lord Waverley was told of his election.



R. W. Gray

ROBERT WHYTLAW-GRAY

1877-1958

Emeritus Professor ROBERT WHYTLAW-GRAY, formerly Professor of Inorganic Chemistry and Administrative Head of the Chemistry Department in the University of Leeds, was the grandson of an Armagh man who emigrated to Australia about a hundred years ago with his wife, daughter and son Robert James, and built up a very big business in Sydney. Robert James Gray returned to Britain as a young man to take charge of the London office, and at the age of twenty-seven he married Mary Gilieson Gemmell, daughter of Robert Adam Whytlaw, of Fenton House, Hampstead, a Glasgow manufacturer of partly Scandinavian origin. The handsome young couple figure in some of the drawings done for *Punch* by their near neighbour, George du Maurier, during the period of their engagement. Their second surviving son, Robert Whytlaw-Gray, was born in London on 14 June 1877.

Whytlaw-Gray received his early education at St Paul's School; very little science was taught in those days, but he carried out chemical experiments in a cupboard in his sisters' schoolroom at home at the age of about twelve. His father wanted him to go into the Army and he sat for the entrance examination, but failed through complete lack of interest, distinguishing himself only by coming out top of the list in chemistry. When he was eighteen he started on an engineering course in the University of Glasgow, where he and his younger sister lived with his grandparents, who now resided there, while his father took his mother and elder sister with him on a business visit to Australia. He there attended lectures by Lord Kelvin, and it was there also that he first heard Sir William Ramsay, who came to the University to give a lecture, and determined to enter University College, London, to study under him. In 1896, his father having returned to London, he was able to do this. The first year's work included chemistry, physics, zoology, botany, and mathematics. Ramsay lectured on four days a week during three terms, but practical chemistry amounted to no more than brief periods of 'test tubing', taught in class by E. C. C. Baly. In 1897 Whytlaw-Gray joined Ramsay's and Collie's classes in physical and organic chemistry respectively, and had a place in the main laboratory. M. W. Travers, who had charge of the laboratory, remembers Baly telling him that 'that good-looking young fellow Gray is the best of the lot'. He proved so to be, and in June 1898 he won the Tufnell Scholarship in chemistry.

Ramsay's method of laboratory instruction consisted partly in setting senior students problems to solve, and in November 1898 Whytlaw-Gray met with an accident in the course of a diazotization which was part of an

attempt at a new synthesis of saccharin, in which Ramsay was interested at that time, probably as a result of a recent visit to his friend Ira Remsen in Baltimore. The after-effects of the accident, not serious in itself, were fairly prolonged; he suffered from gastric trouble during the following months and he did not take the degree examination in 1899 as he would normally have done. As Whytlaw-Gray's parents were in easy circumstances, notwithstanding losses in Australian bank failures, he would in any case, following the custom of the period, have gone on to a German university for a Ph.D. by research, and as Ramsay says in a testimonial written some years later 'he resolved rather to take a German than an English degree, and devoted himself while here rather to research than to examinations'. It was clearly advisable for him to defer his studies in Germany until his health was fully restored, and, while ill-health made his attendance irregular, to get experience in research rather than to attempt to prepare for examinations. When he first returned Ramsay set him to investigate the use of magnesia instead of bone ash for the manufacture of cupels, and as Ramsay said: 'The work led to the foundation of a small but useful industry—the manufacture of "Mabor" cupels for assaying.' This first work of Whytlaw-Gray's, viewed against the background of his subsequent record, is almost as incongruous as Willard Gibbs's first publication, a specification for a railway-coach brake.

In 1901 or thereabouts, Whytlaw-Gray gave a paper to the University College Chemical and Physical Society on recent experimental work on the determination of gas densities. Travers was much impressed by the critical quality of the paper and felt that it indicated a flair for such research. He had been in correspondence with Daniel Berthelot on the latter's method of determining true molecular weights from limiting densities, and was thinking of measuring pressures and volumes of gases by methods which he had worked out for the measurement of low temperatures with constant-volume gas thermometers. He passed on the idea to Whytlaw-Gray, who set to work in a room next to Travers's in the basement of the College, and having mastered the techniques applied himself to a redetermination of the atomic weight of nitrogen. His work justified Travers's confidence in his critical outlook and experimental skill, for he showed that the then accepted value due to Stas was in error while his own value was confirmed by Guye and was adopted as standard by the International Committee on Atomic Weights. The later part of this work was done in Anschütz's laboratory at Bonn, where Whytlaw-Gray had gone in 1903, one of the attractions of Bonn being that the laboratory had a liquid air plant (which had been set up by Travers in January 1903). He presented a thesis on the atomic weight of nitrogen and obtained the Ph.D. degree in 1906.

On his return from Germany in 1906 Whytlaw-Gray was appointed to Sir William Ramsay's staff at University College. In 1908 he became Assistant Professor, and in 1912, during Ramsay's absence in America, he acted for him. In 1910 he was elected a Fellow of University College. During this period he measured physical constants of several gases with a view to the

determination of atomic weights, and collaborated with Ramsay in measuring the physical properties of niton and determining its atomic weight.

In 1914 Whytlaw-Gray resigned his post at University College to take up an appointment as scientific adviser and research director to a company about to be formed to exploit radioactivity or radioactive substances. The declaration of war, however, prevented the incorporation of the company, and Whytlaw-Gray was left without employment. Later in the year he was asked to act as temporary science master at Eton, and as part of the duties was to conduct classes for those about to receive commissions in the army he agreed and started work there in January 1915. Before the end of the first session the use of chlorine by the Germans made it more than ever necessary to teach intending young officers a little chemistry. Later Whytlaw-Gray was appointed a civilian chemical adviser to the Chemical Warfare Committee, and when the Germans started to use toxic smokes in 1917 he commenced investigations on particulate clouds which were soon of great practical value and were the beginnings of a series of studies in this field which were to occupy him for twenty years. As time went on the War Office provided him with assistants, one of whom, J. B. Speakman, accompanied him to Leeds where he eventually became professor of textiles. For his work during this period Whytlaw-Gray was awarded the O.B.E.

He remained at Eton after the war, and the headmaster, Dr Lyttelton, offered him the post of senior science master but he refused it because, in his own words: 'while I regarded this as a tribute to my work at Eton, it would have entailed the expenditure of all my spare time in the organization of the teaching and I would have had to give up research.' In 1923 he applied successfully for the Chair at Leeds which was about to fall vacant by the retirement of A. Smithells and took up his duties as Professor of Inorganic Chemistry and Administrative Head of the Chemistry Department in October 1923. The Chair was not advertised as carrying with it the administrative headship, since the University Council wished to see candidates before a decision about this was made, and in the light of his remark about administration at Eton, which was entirely characteristic of him, it is somewhat curious that he should have accepted the administrative responsibility, apparently without much persuasion since his appointment was settled quite quickly and there is no record of any discussion on this point. It seems likely that H. M. Dawson, who was also devoted to research and was more strong-minded than Whytlaw-Gray, refused to undertake the administration of the department; the third professor, J. B. Cohen, would not have come under consideration because he was due to retire in the following year (when he was succeeded by C. K. Ingold). A year or so after Whytlaw-Gray's arrival the planning of a new building to house the Chemistry Department was begun, and this occupied a good deal of his time until the completion of the building in 1933, but not to the exclusion of his research. One of his first acts on coming to Leeds was to set aside for research purposes a laboratory capable of accommodating about ten workers. Soon the laboratory was full

of members of staff and research students, and the occupants became a very happy family. A large old-fashioned fireplace in one corner would be well stoked up in cold weather, and many discussions on the various investigations took place in the region of its genial warmth; many ingenious solutions of the problems of the day were advanced. Whytlaw-Gray spent a great deal of time in that laboratory advising and frequently taking a hand in the experiments of his research students and assistants, among whom, in the twenties, were C. G. Addingley, G. Child, W. G. Davis, G. Nonhebel, A. L. Roberts, W. Sever, J. B. Speakman and Kathleen Stevenson. Although by present-day standards Whytlaw-Gray wanted very little commercially-available equipment, cathetometers being almost the only major items, he needed a basic minimum of technical services to support him and his students in the construction of their apparatus, and one of his early achievements was to persuade the University Council in 1924 to establish a workshop in the chemistry department and to allow him to employ a mechanic (who was to be paid not more than £3 10s. per week)—a small beginning for which Whytlaw-Gray's successors have cause to be deeply grateful.

Whytlaw-Gray did his fair share of lecturing and in particular always gave the first-year course in inorganic chemistry. He was fortunate in his senior lecturer, W. Lowson, who undertook practically all the administration of the laboratories and the supervision of undergraduate practical courses, and thereby enabled him to devote more time to his post-graduate students and assistants. Their work on atomic weights and particulate clouds, and on the sensitive and accurate apparatus required for studying them, attained widespread recognition, and Whytlaw-Gray was elected to the fellowship of the Royal Society in 1928. The work on clouds was supported by the War Office until the middle thirties, and in 1939 Whytlaw-Gray was once again asked to get together a team of research workers to work on the problems of defence against possible chemical warfare. He and his staff worked very long hours on these problems in addition to their teaching and administration; the work was very successful and the team gained a good reputation, but luckily the need never arose to utilize any of their findings in practice. From 1939 Whytlaw-Gray served as the British representative on the International Committee on Atomic Weights, which continued to function in spite of the difficulties of war-time communications and the inclusion of a German among its members.

Whytlaw-Gray should have retired from his Chair on reaching the age limit in 1942 but continued in office until 1945 when the University conferred the title of Emeritus Professor on him. In spite of his age he was anxious to get back to the measurement of densities and compressibilities of gases and vapours, and this was made possible by an award from the Leverhulme Trustees and by the provision of laboratory space by the University. He was also encouraged to continue his researches by Imperial Chemical Industries Ltd, chiefly as a result of the keen interest taken in his work by Mr M. T. Sampson, who, with many of his colleagues, felt that it

would be a very great loss to the world of science if the accurate methods of measurement developed by Whytlaw-Gray were to be lost after his retirement.

In 1950 the University of Leeds conferred on Whytlaw-Gray the degree of Doctor of Science, *honoris causa*, and in the same year he gave up his work in Leeds and moved to Coventry, in order to be near his daughter. He continued to supervise the work of one of his pupils, Miss D. Massie, in the I.C.I.'s Butterwick Research Laboratories at Welwyn (now known as the Akers Research Laboratories), but he chafed at his inability to continue research personally. Encouraged again by Mr Sampson and Sir Wallace Akers, he went to live at Welwyn Garden City in March 1953, and worked in the Butterwick Laboratories from then until about a fortnight before his death, which took place on 21 January 1958. As a result of his very substantial scientific activities at Welwyn, at an age when most men would expect to enjoy a leisurely retirement, sufficient interest has been aroused to ensure that the fundamental study of the properties of gases and vapours will be continued in this country and that Whytlaw-Gray's highly accurate techniques will not be lost: for this we must be grateful in the first place to Whytlaw-Gray's indomitable spirit and secondly to Imperial Chemical Industries and particularly Sampson and Akers, who provided the means for him to continue his work in congenial surroundings.

Whytlaw-Gray's researches may be divided into two main groups. His first and last love was the investigation of the physical properties of gases and vapours with a view to determining with great accuracy their molecular weights and dependent atomic weights. His second main interest was in the first place more or less forced upon him by events in the 1914-18 war; this was the study of aerosols, particularly the determination of the number and weight distribution of the particles in them as a function of time. Whether this second line of research made a great appeal at first to one who was devoted to fundamental research for its own sake is doubtful, but he put his whole effort into it and as Professor Challenger said in presenting him for an honorary degree in 1950, 'the files of the defence ministries throughout two world wars bear witness to the capacity of this truly academic spirit to apply, at need, all his scientific experience to the problems of his country's defence'.

Whytlaw-Gray was fortunate in that the times and the circumstances of his earlier years threw up problems which demanded talents of the kind which he possessed. He had a passion for accuracy, supported by quite exceptional experimental skill, and, encouraged by Travers who had recognized his promise in this direction, he was soon able to find full scope for his passion in a redetermination of the atomic weight of nitrogen, which was then in some doubt. The manifestly high accuracy of his work tipped the scales in favour of the value 14.01 which later work has modified only to 14.008. At Ramsay's suggestion, his next major effort was the determination of the atomic weight of niton (radon), again a subject about which there was much controversy. The volume of niton available for each determination

was less than 0.1 cubic millimetre, so that the problem was to measure the mass and volume of this minute quantity of gas at known temperature and pressure. The volume was measured in a calibrated glass capillary, and there remained the difficult problem of weighing with sufficient accuracy just under 7×10^{-7} g of the gas. Steel and Grant had just previously described the construction of a micro-balance with which a sensitivity of 4×10^{-9} g could be attained, and Whytlaw-Gray made one successfully, incorporating a few minor modifications. He then carried out the necessary manipulation of the minute volume of gas very beautifully, and after applying a host of corrections arrived at a mean atomic weight for niton of 223. This result was published in 1910; from 1909 till 1914 when he left University College, Whytlaw-Gray carried out a number of investigations with Sir William Ramsay on radium and niton. During the same period he published several papers with Ramsay and F. P. Burt on the physical constants of hydrogen chloride and chlorine, and one on the physical constants of xenon with H. S. Patterson and R. Stafford Cripps (who was later to become Chancellor of the Exchequer).

Whytlaw-Gray probably took no part in chemical warfare research in the first two years of the war, but was then brought in by E. F. Harrison, assistant controller of chemical warfare. When the Germans began to use gas shell which produced a cloud of particles containing toxic organic compounds of arsenic, Harrison, who was responsible for the development and production of respirators, realized at once the need for methods of measuring the concentrations of particulate clouds in order to investigate the efficiency of different filtering materials which might be used in respirators. Smithells and Raper in their obituary notice of Harrison (*Trans. Chem. Soc.* **115**, 562 (1919)) remark that 'in the choice of officers, Harrison's judgement rarely led him astray, and his knack of getting the right man in the right place contributed largely to the success of all his work'; his choice of Whytlaw-Gray to investigate the properties of smoke clouds was certainly an excellent one, for not only did Whytlaw-Gray break new ground in fundamental research, but his investigations were of great immediate value to gas defence and later, when aerosols became important in many other ways, to industry.

With the exception of Tolman's use of the Tyndall meter with its limited scope, and a few observations with the ultramicroscope, little attention had been paid to the study of particulates. Nobody had attempted to count the numbers of particles in clouds, or to study their lifetimes, both essential investigations for the determination of the factors concerned in the production of a cloud and in the development of protection against it. Whytlaw-Gray began by adapting ultramicroscope techniques for the purpose of counting quickly the concentration of particles and following their gradual coagulation. He also devised techniques to determine both the mass of individual particles and their mass concentrations, by means of microfilters which were weighed on a microbalance and by other methods. His techniques

were invariably ingenious, yet simple and elegant; for example, he evolved a neat method of holding each sample stationary in the ultramicroscope cell, long enough to count the particles, by the use of a good, well-lubricated glass stop-cock rotated at a constant speed by means of a gramophone motor. Things which are now readily available commercially, for example, micro-filters, had to be developed in the laboratory; indeed, at least one device now on the market, the Thermal Precipitator, which depends on the fact that a dust-free space exists close to a hot body, was invented and patented by Whytlaw-Gray, H. L. Green, R. Lomax and H. H. Watson during the course of the aerosol work.

Having developed satisfactory methods for measuring the size and number of particles, Whytlaw-Gray studied their rate of coagulation under different conditions. The interpretation of his results was based on Smoluchowski's theory of the coagulation of homogeneous sols, into which he and Patterson introduced certain modifications to make it applicable to gaseous systems. In its modified form it can be applied rigorously to heterogeneous smokes and similar aerial disperse systems. Whytlaw-Gray was constantly experimenting with new methods of producing aerosols and his observations included a wide range of substances. He was particularly interested in the structure of the particles formed by coagulation, which differed widely according to the chemical nature of the cloud and the method by which it was produced. He investigated also the electrification of the particles and the phenomenon of photophoresis in which some particles move towards and others away from a source of light, and he found that very occasionally a particle suddenly changes its sign. There was thus hardly any aspect of the physical behaviour of aerosols which Whytlaw-Gray had not examined during the years he devoted to these studies, and the wide range of his work and his ingenious experimental techniques constituted a major contribution in this comparatively new field. A full account of his work on aerosols and related topics up to about 1931 will be found in the book which he published jointly with H. S. Patterson under the title *Smoke* (Arnold, 1932).

After Whytlaw-Gray's move to Leeds, while the work on aerosols was still going on, he resumed his pre-war interests and set several research students to work on the compressibilities and densities of gases. It took a few years to collect enough data for publication, and the first paper of this kind to appear was one with C. G. Addingley in 1928 on a neat and very accurate method of determining the ratio of the compressibilities of two gases, hydrogen and oxygen being the gases used. 'The atomic weight of xenon' and 'The atomic weight of fluorine' were published in 1931 and 1932 in collaboration with H. S. Patterson and W. Cawood. From 1933 onwards Whytlaw-Gray was in demand as a writer of authoritative reports on atomic weights and in 1939 he was elected a member of the Committee on Atomic Weights of the International Union of Chemistry.

After the second war he returned to the study of gases, but whereas most of his previous work had been concerned with the simple permanent gases he

now turned his attention increasingly to more complex organic gases and vapours. This was characteristic of him; his outlook was never static and he was always pushing his interests outwards and exploiting his genius for experiments. His success was not due only to his experimental skill, great as that was—he was continually applying his critical mind to discover the deficiencies in his subject and in the methods of studying it, and planning ways of overcoming those deficiencies. Although he was trained in the ‘sealing-wax and string’ era he was never satisfied to rely on the techniques of the past, no matter how successful they had been. It was, for example, a considerable achievement in his early work on niton to make a balance sensitive to 2×10^{-9} g; but in later years when he and his co-workers required many such balances, he simplified and improved the design very much. In his final work the balance beam was of very simple design and mounted on a very thin silica torsion fibre. This avoided the necessity to grind very perfect knife edges, and also enabled a balance to be made of such small dimensions that it could be slipped into a small all-glass case made out of tubing of standard bore (about 70 to 80 mm). The balance could always be made more sensitive than other measurements justified, so Whytlaw-Gray was always giving thought to other factors which caused trouble and experimenting to find out ways of improving accuracy. For many years he was anxious about errors in volume measurements due to the variation in shape of the mercury meniscus and the effects of capillary depression. In later days he and N. Teich carried out an extensive investigation of this factor. One difficulty always present was the reading of a manometer, which could not be done with greater accuracy than about 0.01 mm of mercury. In the end, as the microbalance was a more sensitive measurer of pressure, Whytlaw-Gray used the microbalance to measure the pressure difference between two equal volumes of gas at the same density; in this way he was able to measure a difference in pressure of 5×10^{-4} mm of mercury. He also devoted much thought to the elimination of errors due to the adsorption of gases and vapours on the walls of the containing vessels, and a note to *Nature* containing a reference to this subject, which he sent off a few weeks before his death, shows how to the last he was searching out and removing obstacles to the attainment of higher accuracy in the measurement of the properties of gases.

Whytlaw-Gray carried out several researches on topics other than gases and aerosols; among them should be mentioned his studies with K. G. Denbigh of the fluorides of sulphur. Referring to this work in his lectures on modern inorganic chemistry to the Institute of Chemistry in 1933, G. T. Morgan remarked that researches on the interaction of elemental fluorine with other substances had hitherto been carried out mainly by foreign chemists, and expressed his pleasure at Whytlaw-Gray’s incursion into this field. In retrospect, knowing now the importance of fluorine chemistry and the relative unimportance of ‘poison gas’ in World War II, one cannot but feel sorry that Whytlaw-Gray did not extend his fluorine work and that his

exceptional talents were not called on to advance it still further during the war.

In person Whytlaw-Gray was slightly built, somewhat below middle height, while his general appearance, with his brisk gait and neat dress, can best be described as 'dapper'; in his youth he was very goodlooking. The outstanding thing about him, which remains in the minds of all who knew him, was his enthusiasm for his subject, which he retained undiminished until his death. During a conversation on a chemical subject he would listen, bird-like, head on one side and lips parted, ready to pounce eagerly on any new idea which interested him. He delighted in the exercise of experimental skill, and in ideas for new experiments; he was amazingly skilled as a glass-blower, perhaps partly because, as his brother-in-law, M. W. Travers, has said: 'When it came to designing a balance to weigh nothing at all, the line Ramsay seemed to take was "we've got to do it and you can do it, so just get on with it and don't worry me".' Whytlaw-Gray himself tempered the wind to his pupils; as a teacher in the laboratory he excelled because he could do all that he taught, and he was always ready to demonstrate any experimental operation, even when this was so difficult that complete success could not be assured. If he failed it did not disturb his urbanity and his strongest expletives were 'By George' and 'Dear me, how tiresome'. His words of commendation were equally restrained, his highest praise for an idea or an experimental achievement being 'Very dodgy'. Under his tuition all his research men became good glass-blowers. Almost invariably as a first exercise he set each of them to build a vacuum train including a Töpler pump—all in soda glass. When he retired, so many Töpler pumps were collected from his laboratories that it was impossible to store half of them.

Whytlaw-Gray had a casual way of going about things, and, perhaps strangely in one dedicated to work of the highest precision, he was rather untidy both in the laboratory and in his paper work: nevertheless he could produce a wealth of reliable experimental results in a very short time. He was not fond of administration and his normally open countenance would assume a distant and evasive cast if the turn of a conversation suggested to him that he might be called upon to leave his laboratory bench for some office work; important and urgent papers sometimes disappeared for a considerable time under the pile of papers on his table. His attitude to office work of any kind was well shown by the design of the inorganic part of the new chemistry department in Leeds, for which he was responsible; except for his own private room he allocated not one square foot of floor space to private rooms and offices, but turned it all into laboratories where experimental work could be done. True, he did allow a little space for stores, workshop and library, but these were necessary adjuncts to the laboratories. Because he was unwilling to give time to hard thinking about organizational problems, and perhaps also because he was by nature courteous and gentle and averse from conflict, he did not exercise a very positive leadership as administrative head of the chemistry department; the first reason is probably

the principal one because his modest unassuming manner concealed a shrewd and decisive character, and he could be quite astonishingly authoritative when the occasion warranted. He represented his department faithfully in the Senate, where he was universally regarded with affection, but he took no part in general University administration or in the shaping of academic policy.

Outside the laboratory Whytlaw-Gray's interests lay in his family, in nature, and in photography. He had a great knowledge and love of nature generally and in particular a passionate interest in hills and hill scenery which was first aroused by mountain walking in Scotland in his youth. In consequence he was a delightful and informative companion to his children and others on country walks and mountain rambles, and a source of pleasure to many friends to whom he sent Christmas cards adorned by his own landscape photographs.

In 1911 Whytlaw-Gray married Doris Fortescue, daughter of Arthur Fortescue Carr, who survives him. They had two daughters, one of whom, Phillipa Mary, obtained a Ph.D. for work done under her father's direction and married another of his pupils, Dr G. A. R. Hartley. To a younger generation the Whytlaw-Grays may have appeared a rather Victorian couple at home, but they were devoted to each other for nearly fifty years and in the last year or two, when Mrs Whytlaw-Gray was an invalid, her husband was unwilling ever to be more than a few minutes' journey away from her. He finally decided, with extreme reluctance, that he could no longer nurse her himself and at the very moment when the ambulance came to their home to make the separation he collapsed. Though he rallied several times he never was well enough to leave his bed at hospital and he died after an illness of a little over a fortnight.

We are indebted to many of Whytlaw-Gray's former colleagues and pupils for writing to us about him, and we particularly wish to thank Mrs G. A. R. Hartley, Dr M. W. Travers and Sir Harold Hartley for providing much valuable information about his parentage and his life and work before he came to Leeds.

E. G. COX
J. HUME

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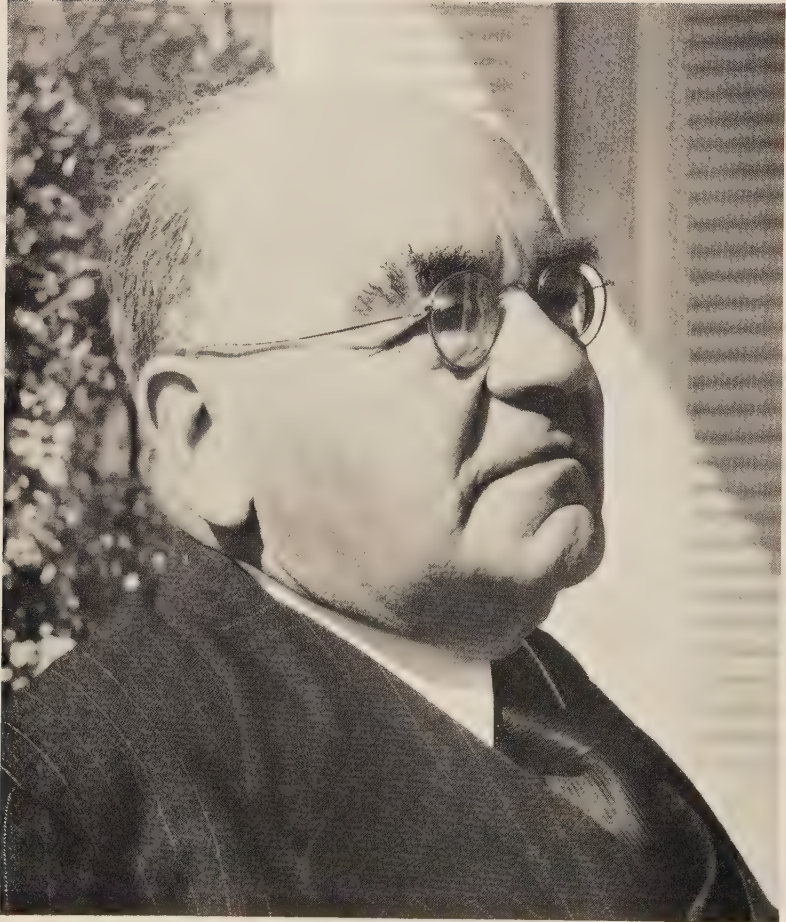
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Whiteland

HEINRICH WIELAND

1877-1957

HEINRICH WIELAND, whose death took place on 5 August 1957 at his peaceful home in Starnberg, belonged to the classical age of organic chemistry. But it was not only in this field that his influence was so strongly felt; the younger science of biochemistry also owes much to him, for his dehydrogenation theory marked a major advance.

Wieland was born at Pforzheim (Baden) on 4 June 1877 and received his early education in his home town. After obtaining his school-leaving certificate from the local grammar school in 1896, he spent two semesters studying chemistry at the University of Munich and the winter semester of 1897 at the University of Berlin. This was followed by a further year of study at the Technical High School in Stuttgart, after which he returned to Munich, where he was awarded a doctor's degree in 1901, at the early age of 24. Three years later he qualified as a lecturer at the same University, and in 1909 he was appointed Associate Professor. Four years later he accepted a position as full Professor (*ordinarius*) at the Technical High School in Munich. In 1921 he moved to the University of Freiburg (Breisgau), where a broad and interesting field of research awaited him. But although his work there brought him much satisfaction, the climax of his career came in 1925 when his own University of Munich offered him the chair of organic chemistry, previously held by R. Willstätter, Adolf von Baeyer and Justus von Liebig. He accepted, and here he remained until he reached the age of retirement at the end of the war.

As a research worker, Heinrich Wieland applied his experimental skill to many different branches of organic chemistry, and in almost all his work he was breaking new ground, tackling problems to which, previously, hardly anybody had paid any attention. His studies of the bile acids, the toad poisons, the alkaloids of lobelia and curare, the poisons of the deadly amanita, the pterins, and his work on biological dehydrogenation and many other subjects, all had to be undertaken without any earlier experimental work as a basis on which to build. It is therefore understandable that Wieland first had to find and develop new methods of approach and that he was sometimes led astray by concepts based on a false interpretation of his results.

During the first few decades of his scientific work, Wieland devoted himself to an intensive study of various *organic nitrogen compounds*, his interest in which had no doubt been awakened by his teacher Johannes Thiele. First he investigated the addition of the nitrogen oxides N_2O_3 and N_2O_4 to olefines

and elucidated the constitutions of the addition products. The results of these studies on the addition of nitrogen oxides to carbon—carbon double bonds led Wieland to formulate the dimeric nitrogen oxides as possessing N—N— linkages, i.e. as ON—NO_2 and $\text{O}_2\text{N—NO}_2$, and to compare them with the halogens.

A thorough investigation was made of the tetra-arylhydrazines, and their formation from diphenylamines by oxidation processes, as well as their behaviour towards anhydrous acids, were examined more closely. The thermal dissociation of tetra-arylhydrazines into free diarylnitrogen radicals, which can be captured, for example, by NO , was first observed in 1911. From a large number of such observations Wieland concluded that this irreversible dissociation is dependent upon the substitution of the benzene nucleus and is favoured by positive substituents.

By cautious oxidation of diarylhydroxylamines with silver oxide, Wieland obtained diaryl nitrogen oxides of the general formula R_2NO , which he recognized as radicals, although they did not show any tendency to dimerization. Studies were also made on the action of alkyl magnesium salts on nitroso compounds, on the preparation of nitrile oxides, $\text{RC}\equiv\text{N}\rightarrow\text{O}$, from nitrol acids by splitting off nitrous acid, and on the dimerization of these oxides to furoxanes. From benzhydroxamic acid chloride and hydrazine it was possible to obtain hydrazide oximes which yielded derivatives of *N*-hydroxytetrazole on treatment with nitrous acid. These were the first tetrazole compounds to be discovered.

Wieland made an extensive study of fulminic acid. He elucidated the reaction mechanism of the well-known method of preparing this substance from ethanol, nitric acid and mercury, and showed that all the intermediates postulated for this reaction could be converted into fulminic acid. He was also able to clarify the long familiar polymerization of fulminic acid and to show that in the primary reaction only two polymeric fulminic acids are formed, the trimeric metafulminic acid and the tetrameric *isocyanilic* acid. Both these acids were synthesized by routes which left no doubt as to their constitution. More than 90 papers were published by Wieland and his pupils describing these studies on nitrogen compounds.

Wieland's interest in the *bile acids*, which absorbed him for more than 20 years, dates back to 1912, a time nothing of importance was known regarding the constitution of the steroids. Without the aid of techniques today considered indispensable to organic chemistry research—ultra-violet and infra-red spectrometry, chromatography, X-ray analysis, etc.—and using only the methods of classical organic chemistry, he attempted to penetrate into the structure of a class of compounds which offered great difficulties to the research worker, and which he himself once described as a 'dürre Strukturwüste' ('arid structural desert'). He published 38 papers devoted to the results of these investigations and describing the various new methods of degradation which he developed for breaking down the carbon rings of the steroid skeleton. In this work he undoubtedly derived valuable

support from the research on the structure of cholesterol and other sterols which was being carried out at the same time by his friend and colleague Adolf Windaus, for one group of research workers could often profit from the experience of the other.

From ox bile, Wieland isolated the cholic acid $C_{24}H_{40}O_5$ and desoxycholic acid $C_{24}H_{40}O_4$. He was able to show that both these acids had the same carbon skeleton, since they could both be converted into the same cholanic acid $C_{24}H_{40}O_2$ by splitting off water and subsequent hydrogenation. Later, new bile acids, with the hydroxyl groups in different positions, were isolated from human bile and from goose and pig bile. Then, in 1919, Windaus succeeded in degrading cholesterol to cholanic acid by the removal of water, hydrogenation of the anhydro-derivative, and subsequent oxidation with chromic acid. The relationship between the bile acids and the steroids was thus demonstrated and there could no longer be any doubt that all these compounds had the same carbon skeleton.

Wieland and his pupils then set about the stepwise degradation of the bile acids, a task that was made easier by differences in the reactivity of the various OH-groups. It was found that reactions proceeded most readily with the OH-group in ring A, while the OH-group attached to the carbon atom nowadays referred to as C_{12} was the most difficult to attack. It was possible, for example, to obtain lithocholic acid from desoxycholic acid. At first, however, no definite conclusions regarding the size of the carbon rings could be drawn from these degradation experiments. In the early experiments, great reliance was placed on Blanc's rule, according to which 1 : 6- and 1 : 7-dicarboxylic acids should give 5- or 6-membered rings on distillation, but later work showed that this was not always the case. It held for the degradation of ring A but not for ring C, which was at first thought to be five-membered. Further experiments were aimed at determining the position in which the aliphatic side-chain of the sterols was attached.

In the middle of this feverish research activity, there appeared a paper by Rosenheim, who was able to put forward a new formula for the steroids, based on the results of X-ray studies. It was at once apparent that, after slight modification, this formula readily explained all Wieland's experimental results. The basic problem of steroid research was solved. From that time onwards, Wieland scarcely took any further part in research on this group of compounds, apart from one or two publications on the minor sterols of yeast. Even so, his work on the bile acids played such an important role in the subsequent development of steroid chemistry that he received the Nobel Prize for it in 1927.

In the course of his work on the bile acids, Wieland also discovered the so-called choleic acids, compounds formed by the combination of bile acids, especially desoxycholic acid, with higher fatty acids, fats, hydrocarbons and similar compounds which give colloidal solutions in water. Apparently one of the physiological functions of the bile acids is to render fats water-soluble by converting them to choleic acids, thus making them accessible to the

digestive processes. Wieland was also the first to prepare methyl cholan-threne, a substance that was later found to be strongly carcinogenic.

From 1913 onwards, at the same time that he was working on the bile acids, Wieland also investigated the toad poisons. These exert an action on the heart similar to that of the cardiac glucosides. From the cutaneous secretions of *Bufo vulgaris* he isolated bufotoxin, which proved to be the suberyl-arginine ester of a genin, bufotalin. On heating with acids, both the genin and the toxin yielded the same substance, bufotalien; this could be degraded to a cholanic acid, $C_{24}H_{40}O_2$, different from any of the known cholanic acids. Finally, the structure of the toad poison was elucidated completely, and it was found that bufotalin differs from the bile acids mainly in the structure of the side-chain, which is present in the form of an unsaturated δ -lactone ring.

Still another of the polycyclic compounds studied by Wieland was quinovic acid, found in the mother liquors from the manufacture of quinine. This could be shown to be a compound containing 5 six-membered carbon rings. The linkage of the rings was established by dehydrogenation with selenium, which yielded 1:8-dimethyl-picene.

Wieland began to take an interest in the *alkaloids* as early as 1911, when he made a study of the plant base morphine and succeeded in shifting the position of the double bond in the molecule. This work was continued by his pupil C. Schöpf, who was able to confirm, among other findings, the correctness of R. Robinson's assumption that the nitrogen-containing side-chain is attached to the quaternary carbon atom.

The alkaloids of *Lobelia* were the subject of an exhaustive study by Wieland. He succeeded in isolating from *Lobelia inflata* a large number of new bases, all of which were derivatives of piperidine, and he was able both to establish their constitutions and to synthesize them. Members of the so-called lobeline group include: lobeline (a keto-alcohol), lobelanine (the corresponding diketone), lobelanidine (the corresponding di-alcohol), nor-lobelanine, and nor-lobelanidine. The constitution was established by Hofmann degradation to 1:7-dibenzoylheptane, $C_6H_5CO(CH_2)_7COC_6H_5$, as well as by energetic oxidation to scopolinic acid (*N*-methylpiperidine- α , α' -dicarboxylic acid). All these alkaloids were synthesized in the racemic form, and some in optically active form as well. Lobeline is today synthesized on a technical scale for use in medicine; it stimulates the respiratory centre and is employed to facilitate and strengthen respiration.

The second group of *Lobelia* alkaloids, the so-called lelobine group, includes the 4 alkaloids rac. lelobanidine, (—)-lelobanidine I, (+)-nor-lelobanidine and (—)-lelobanidine II. They differ from the first group in the carbon skeleton, and are di-alcoholamines. It was also possible to establish the constitutions of a third group of *Lobelia* alkaloids—the lobinine group. This consists of 4 bases, two of which are keto-alcohols (lobinine and isolobinine) and two di-alcohols (lobinanidine and isolobinanidine). They all contain a carbon-carbon double bond.

The *Strychnos* alkaloids also held Wieland's attention for many years, and 27 publications are devoted to his work in this field. His interest in these compounds was awakened by the discovery of vomicine by his pupil E. Gmelin. The seven-ringed structure of the strychnine molecule presented such great difficulties to the research worker, however, that Wieland was unable to pursue his studies to the point of establishing the correct constitutional formula. Nevertheless, he was able to isolate numerous degradation products which are still of considerable interest today from several points of view.

Between the years 1937 and 1943, Wieland also carried out the first successful investigation of calabash-curare, isolating from this substance and from the bark of *Strychnos toxifera* a number of crystalline and homogeneous alkaloids (C-curarine I, II and III, C-toxiferine I and II, toxiferine II, C-dihydrotoxiferine I and C-isodihydrotoxiferine). Some of these quaternary ammonium salts exhibited a very powerful curare-like action and a remarkably high toxicity. Although 20 years have now passed, their constitution has still not been completely elucidated.

Another group of very toxic compounds in which Wieland was interested comprised the poisons of the fungus *Amanita phalloides* (deadly amanita). He was able to isolate two crystalline components of the poisons amanitin and phalloidin, the latter with the help of his associates F. Lynen and U. Wieland. Phalloidin was thought to be a hexapeptide, consisting of 1 molecule of cysteine, 2 molecules of alanine, 2 molecules of 2-hydroxyproline, and 1 molecule of α -hydroxytryptophane. However, later studies by his son, Theodor Wieland, showed that this polypeptide has a somewhat different composition, since on hydrolysis it yielded 2 molecules of L-alanine, 1 molecule of D-threonine, 1 molecule of L-allo-hydroxyproline, 1 molecule of L-hydroxytryptophane, 1 molecule of L-cysteine, and 1 molecule of L- γ , δ -dihydroxyleucine.

The glandular secretions of various species of toad contain a number of basic substances in addition to the steroid lactones, of which mention has already been made. Wieland was able to isolate bufotenine, bufotenidine and bufothionine and to show that they are indole derivatives. Bufotenine was characterized as *N*-dimethyl-5-hydroxytryptamine, i.e. a derivative of tryptophane, and bufotenidine as its (internal) quaternary salt.

Wieland's experimental skill showed itself to particular advantage when, in the 1920's, at the suggestion of his pupil C. Schöpf, he turned his attention to the *insect pigments*, and especially to the *pterins*. It is true that as long ago as 1889-91, F. G. Hopkins had crystallized the white pigment of the cabbage-white butterfly and somewhat later had isolated the yellow pigment of the brimstone butterfly in an amorphous, still impure condition. But these discoveries attracted little attention and were not taken up, largely on account of Hopkins's erroneous conclusion that the pigment of the cabbage-white was identical with uric acid. In 1925, with a team of highly gifted associates—in particular C. Schöpf, R. Purman and W. Koschara—

Wieland began an extensive study of the butterfly pigments. From the brimstone butterfly (*Gonepteryx rhamni*) he was able to isolate the yellow pigment xanthopterin, and from ten thousand cabbage whites (*Pieris rapae* and *Pieris napi*) he obtained the main pigment leucopterin, which he immediately recognized as distinct from uric acid. It could also be shown that leucopterin was accompanied by small quantities of other pigments, including desoxyleucopterin and a red pigment, to which the name erythropterin was given.

The wide distribution of these natural products, the pterins, in the animal and vegetable kingdoms could later be demonstrated; they were found in cattle, rabbits, and horses, in plants, and as a normal component of human urine. It is now known that they are ubiquitous in living organisms and that certain pterins—the number of known compounds increases from year to year—exercise important physiological functions. It is recognized today that the formation of several of the pterins is controlled by hereditary factors. Another pterin derivative is the vitamin pteroylglutamic acid or folic acid, a necessary auxiliary factor in the diet of animals, man and many micro-organisms.

With characteristic perseverance, Wieland also solved the problem of the constitutions of the most important of the then known pterins, a task that was more difficult than would be expected from the relatively simple structures of these compounds. For a long time, unusual analytical difficulties and ambiguity in the interpretation of the experimental results prevented him from putting forward well-founded formulae. Eventually, however, all these difficulties were overcome and the structures of leucopterin, xanthopterin and isoxanthopterin (8-desoxyleucopterin) were confirmed by synthesis.

In the light of what is known today regarding the pterins, it can scarcely be doubted that they will be shown in the future to play an even more important role in biological processes and that new physiological actions will be discovered.

Wieland's studies on dehydrogenation have been left to the end of this account of his research work for the simple reason that the problems concerned and the techniques employed were essentially different from those of his other investigations, most of which were devoted to structural questions. In actual fact, his work on dehydrogenation extended over practically the whole period of his research activity, from 1912 onwards.

The combustion processes taking place in the living cell were interpreted by many physiologists prior to 1912 as an activation of oxygen: it was believed that combustion involved the action of a respiratory enzyme, or that the active oxygen appeared in the form of peroxide (theory of Engler and Bach). According to the new idea put forward by Wieland in 1912, combustion processes are due to an activation of the substrate, leading to a weakening of the hydrogen bonds and subsequently to a splitting off of hydrogen atoms. Even this theory was not entirely new, however. Similar views had been expressed by Schmiedeberg (1881), M. Traube (1882) and Pfeffer (1889),

but they were not able to gain acceptance for them as they were not supported by convincing experimental evidence.

Wieland now set about obtaining an experimental basis for his dehydrogenation theory. His first aim was to find out whether catalytic hydrogenation with palladium might be reversible, and he did, in fact, succeed in dehydrogenating a number of reactive, organic hydrogen compounds, such as dihydronaphthalene and dihydroanthracene, by means of palladium. In the absence of oxygen, water-containing or moist aldehydes were converted by palladium into carboxylic acids, a process that was interpreted as dehydrogenation of the aldehyde hydrate. That elementary oxygen is not essential for 'oxidation' processes was demonstrated by experiments in which ethyl alcohol could be dehydrogenated to acetaldehyde by the action of quinone or methylene blue alone, the latter substances acting as hydrogen acceptors.

In an attempt to develop model reactions for biological dehydrogenation processes, a thorough study was made of the oxidation of a variety of organic and inorganic compounds which could be interpreted as dehydrogenations, e.g. the catalytic removal of hydrogen in the presence of palladium from phosphorous acid, hypophosphorous acid and formic acid. The combustion of carbon monoxide was interpreted as the dehydrogenation of formic acid, formed as an intermediate, and the oxidation of indigo as a dehydrogenation to dehydroindigo, followed by rearrangement and decomposition into indigo and isatin.

With the aim of substantiating his dehydrogenation theory, Wieland made a particularly intensive study of the dehydrase systems of living organisms. Vinegar bacteria, Schardinger's enzyme from milk (xanthine dehydrase), and the dehydrases of muscle tissue and yeast were the enzymes he most often used. He was able to demonstrate conclusively that the oxidation of aldehydes in the presence of Schardinger's enzyme takes place in exactly the same manner, whether the oxidizing agent is oxygen or methylene blue. Since methylene blue can be used for this reaction, it can only be interpreted as a dehydrogenation. Another surprising and important observation was the conversion of acetates to succinic acid by means of oxygen-starved yeast, again a process that can only be regarded as a dehydrogenation.

At about the same time that Wieland began his studies on dehydrogenation, Otto Warburg also turned his attention to oxidation processes (1914). In contrast to Wieland, however, he attributed the oxidation processes in living organisms not to the activation of hydrogen, but to the activation of oxygen by a metal, especially by iron, and in many cases he was able to prove the correctness of this theory. During the years that followed, there developed a very lively controversy over the two views. This was perhaps not always very pleasant for the two research workers, but it undoubtedly had the happy result that biological oxidation processes were subjected to a very intensive study, leading to numerous important discoveries. It is now known that both substrate-activation and oxygen-activation play a part in

cellular combustion, and that often there is a combination of the two processes, brought about on the one hand by dehydrases and on the other by oxidases. There can be no doubt, for example, that nicotinic acid amide, adenine nucleotide (codehydrases I and II) and the flavine enzymes are true dehydrases which remove hydrogen from the substrate, whereas cytochrome oxidase, peroxidase and catalase are enzyme systems which bring about an activation of oxygen. According to Warburg, sensitivity to hydrogen cyanide, due to inactivation of iron, is a characteristic of aerobic enzymatic processes, while anaerobic processes (fermentation reactions) are insensitive to cyanide.

Thus, both Wieland's theory of the primary activation of hydrogen and Warburg's theory of the activation of oxygen have their validity for biological phenomena. Both made a decisive contribution to the concept of cellular combustion in its present form—a form shaped largely by the two theories and the discoveries that sprang from them. These studies therefore constitute part of the basis of modern biochemistry.

In a life devoted to scientific research, Heinrich Wieland brought about far-reaching changes in many branches of organic chemistry, particularly in the chemistry of the natural products, as well as in the field of biochemistry. The outstanding quality of his work was fully recognized by his colleagues. It has already been mentioned that in 1927 he was awarded the Nobel Prize for Chemistry. Honorary degrees were conferred on him by the Technical High School, Darmstadt (Dr.Ing.), the University of Freiburg (Dr.Med.), and the University of Athens (Dr.phil.). He was a Foreign Member of the Royal Society, of the National Academy of Sciences, Washington, and of the Academy of Arts and Sciences, Boston; an Honorary Member of the Chemical Society of London, the Roumanian Chemical Society, the Japanese Academy and the Indian Academy; a Member of the Academies of Munich, Göttingen, Heidelberg, and Berlin; and a Corresponding Member of the Academy of Sciences of Moscow. His home country made him a Member of the 'Friedensklasse des Ordens pour le Mérite' and awarded him the 'Grosse Verdienstkreuz der Bundesrepublik', and in 1955 he was the first to receive the Otto Hahn Prize.

This portrait of Heinrich Wieland would not be complete without some reference to the human side of his character. He was a man of remarkable energy and had an unusual capacity for work, coupled with a penetrating intelligence. It was not only to these qualities, however, that he owed his success as a scientific worker and an academic teacher. A great deal must be attributed to his personality. An even temperament, cool judgement, and a strong sense of justice were prominent characteristics. But equally remarkable was his modesty, which, springing as it did from a deep conviction, he was able to preserve into his old age, in spite of all his successes. This modesty prevented him from pushing himself forward, and it was rare that he even visited congresses. He loved quiet and the small circle of friends that he had gathered around him through his attractive personality. In such surroundings, the inherent gaiety of his disposition, restrained rather than boisterous,

would reveal itself, and from time to time he was even known to throw in a sarcastic remark. A quiet humour lay hidden beneath his outward seriousness and was often revealed by an apt remark, or perhaps only by a twinkle in his eye.

In addition to science, Heinrich Wieland also took an active interest in the arts, especially in painting and music. He often had musical gatherings in his home, and in his younger days he himself would join in. In a quiet way, he helped many an artist in a difficult situation.

Wieland did not take part in politics, but at heart he was a convinced democrat. His sense of justice brought him into sharp opposition to the third Reich, whose ideologies and actions he uncompromisingly rejected. During those years when opposition was bound up with personal danger, he courageously stood up for right, human values, and freedom of speech, and more than once he used his personal influence to help those who were persecuted.

His greatest concern, however, was always for his family. He was happily married, and with his wife Josephine (*née* Bartmann) he had the satisfaction of watching their four children—three sons and a daughter*—grow up into independent citizens and achieve success in their professions. He was especially appreciative of the fact that, through his grandchildren, who were very attached to him, he was able to keep in touch with young people, even into his old age. His daughter's children grew up in a neighbouring house in Starnberg and came to visit their grandfather almost every day.

In the last years of his life his eyesight began to fail, but his keen mind remained alert to the last. Even after his retirement, he continued to follow the progress of chemistry, and he was personally responsible for editing the *Annalen der Chemie* until shortly before his death.

The death of Heinrich Wieland brought to an end a rich and harmonious life. His work will live on, however, and its fruits will continue to enrich many branches of science.

P. KARRER

* Dr Wolfgang Wieland, Professor Dr Theodor Wieland, Eva Lynen *née* Wieland and P.D. Dr med. Otto Wieland.

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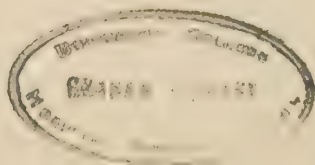
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John C. Willis—

JOHN CHRISTOPHER WILLIS

1868-1958

JOHN CHRISTOPHER WILLIS was born at Birkenhead on 20 February 1868. He studied at University College, Liverpool, and at Cambridge and for a time was an assistant in the Botany Department at Glasgow. In 1896, he was appointed director of the Royal Botanic Garden, Peradeniya, Ceylon, and held the post for 15 years. From 1912 to 1915 he was director of the Botanic Garden at Rio de Janeiro and after his retirement he worked at Cambridge and later went to live at Les Terrages, Avenue des Alpes, Montreux, Switzerland, where he died on 21 March 1958. He married Minnie, daughter of T. Baldwin, in 1897, and she died in 1931. There were three daughters of the marriage. He was an M.A. and Sc.D.(Cantab.), and was given an honorary S.D. by Harvard. He was elected a Fellow of the Linnean Society in 1897 and a Fellow of the Royal Society in 1919. *The Annals of the Royal Botanic Gardens, Peradeniya*, was a periodical founded by him, volume 1 part 1 appearing on 27 June 1901. Willis contributed very largely to the earlier volumes and edited volumes 1 to 4 inclusive, to 1910, and volume 5, parts 1 to 3, to December 1911. He was also editor of *The Empire Cotton Growing Review* from vol. 1, 1924 to vol. 16, 1939.

In his earliest botanical researches Willis showed himself an excellent observer and describer as his published papers on pollination and dispersal bear witness. His appointment to Peradeniya resulted in a series of studies on the Podostemaceae, a family of plants living in tropical rivers and showing remarkable features in structure and behaviour. The plants grow in rocks in rushing water that generally rises and falls periodically. The morphology of the vegetative organs is varied and complex. The primary axis is usually small and from the base there buds out a green thallus, most often of adventitious root nature, which may be filamentous, flattened, creeping, or ribbon like. In one sense the species are highly adapted to a peculiar mode of life. Willis, however, was greatly impressed by the fact that the fairly considerable number of genera (about 22) and species (over 100) grow under similar conditions, that many have restricted ranges, and yet they show great diversity. On the smooth rocks in rushing tropical and subtropical mountain streams 'there is nothing to which the many genera and species can be adapted, for the conditions are the same for all, and could not be equalled for uniformity in a laboratory of the temperate zone. They grow only upon a smooth rigid substratum, from which they take no food; all grow in water, and have no climatic differences, no difference in circumambient medium, in light, or in any other factor. And yet they have evolved many genera and

species, with very striking and bizarre differences between them. Evidently it is not necessary to have local conditions to which to be adapted in order to ensure that evolution shall go on' (*Age and area*, p. 57). Diverse species and genera sometimes grow together and the family seems 'to go on producing new forms in extraordinarily uniform conditions' (*Birth and spread of plants*, p. 346).

In 1905 an accident affected Willis's eyesight and he turned to work with the aid of a pen and library and largely devoted himself to the study of geographical distribution by analyzing published floras. That Willis had a remarkable talent for compiling, abstracting, and condensing was shown, before his accident, in the preparation and publication of *A manual and dictionary of the flowering plants and ferns*, the first edition of which appeared in 1897 in two handy volumes. Later the very useful general part was more or less incorporated in the *Dictionary* proper—an alteration that was in some ways unfortunate. Five more editions appeared and the last (1931) has been several times reprinted up to the present date. A few years ago Willis presented all his rights in the *Dictionary* to the Bentham-Moxon Trustees of the Royal Botanic Gardens, Kew, and a new edition is in course of preparation at the Kew Herbarium and should be published at Cambridge by the University Press within a few years. Very few botanical works of this size can be so useful as Willis's *Dictionary*. The amount of information it gives is enormous and it has a high standard of accuracy. Many working botanists keep it handy for frequent reference and it was 'favourably received among travellers, residents in outlying districts, schoolmasters, and the considerable class of people who have an indirect interest in botany and need some work of general reference in that subject' (Preface to ed. 3). Another, and non-botanical, example of Willis's interest in compiling and arranging facts is *The tube-bus guide to London*, published in 1928 which was designed to make a stranger to the metropolis 'comparatively independent of outside aid' in finding his way to almost every place of general interest in London. Most botanists would find the preparing of such a booklet intolerably monotonous but it surely indicates the kind of mentality which delighted in compiling numerous tables from published works and working them up in support of theories of plant geography.

Relatively early in his life as a professional botanist Willis became dissatisfied with the theory of natural selection as an explanation of the mechanism of organic evolution. From time to time in a series of papers commencing with those on the Podostemaceae already mentioned he published what he considered objections to it. A very direct attack with a suggested replacement of natural selection as an inclusive theory by what he termed 'age and area' was initiated in 1916 in a paper dealing with the flora of Ceylon. This paper was followed by a series of others along similar lines for other aspects of the Ceylon flora and for that of New Zealand. These papers were used, with much additional material, in his first book on the subject, published in 1922 under the title of *Age and area*, and most of Willis's

subsequent activities were concerned up to late in his life in supporting and extending the conclusions as stated in *Age and area*. In the simplest terms the working hypothesis is that the range of a taxon depends on the age of the taxon and conversely that range gives a clear indication of age. The word taxon is used because, though Willis first applied age and area to species, he later extended it to genera and families. To meet some of the criticisms which were made of his earlier statements on age and area he gave, in 1919 and reprinted in 1922, the following 'expression of the rule': 'The area occupied [determined by the most outlying stations] at any given time, in any given country, by any group of allied species at least ten in number, depends chiefly, so long as conditions remain reasonably constant, upon the ages of the species of that group in that country, but may be enormously modified by the presence of barriers such as seas, rivers, mountains, changes of climate from one region to the next, or other ecological boundaries, and the like, and also by the action of man, and by other causes'. He claimed that this 'appears to be a general law covering all or nearly all the plants now existing upon the globe, and to have determined their distribution thereon, in broad outline'. It has been pointed out that this 'expression' cannot be tested as it stands. 'Willis has so tied up his hypothesis in an endeavour to make it impenetrable to criticism that it has become unworkable in the form in which it is expressed' (Turrill, *Plant-life of the Balkan Peninsula*, p. 466, 1929). Willis himself and others who have, at least in part, accepted the hypothesis that on the whole a larger distributional area or range is a criterion of greater age in a given country, in practice ignored the conditions laid down in the expression quoted above. Another principle derived by an extension of that of age and area is 'size and space', that on the whole, keeping within the same circle of affinity, a group of large genera 'will occupy more space than a group of small'. It follows that size goes with age and the 'laws' of age, size, and area (ASA) explain distribution.

As a main basis for his attack on the Darwinian theory of natural selection, Willis accepted mutations, but apparently mainly relied on the occurrence of large mutations, such that 'a single mutation, usually very divergent from the parent form, may give rise, at one step (not gradually as under Darwinism) to a new form, of family, generic, specific, or varietal rank' (*The birth and spread of plants*, p. 14, 1949). It is a strange paradox that mutations are generally considered the basis of natural selection as understood by modern evolutionists (see, for example, J. Huxley, *Evolution, the modern synthesis*, 1942). Apparently relying largely on the occurrence of large mutations, Willis accepted the theory of divergence or differentiation, first proposed by Guppy in 1906, that evolution did not proceed from individual to variety, from variety to species, from species to genus and from genus to family, but inversely, the great families and genera appearing at a very early period, and subsequently breaking up into other genera and species. In Willis's hands this theory was developed somewhat in the form of dichotomous divergent mutation.

What he called 'the hollow curve of distribution' is greatly stressed by Willis: that if, for example, genera of a family or of a geographical area are plotted on a graph according to the number of species they contain a curve which is concave upwards is obtained, there being many genera with only one species, fewer with two, and so on, the drop being often sudden. He argued that these curves could not be reconciled with the theory of natural selection but that distribution is in general what one may call very largely accidental, and not determined by adaptation in so far as concerns general distribution about the world. He concluded (*Age and area*, p. 199): 'The hollow curve is apparently a universal principle of distribution, whether it be distribution in space—geographical distribution—or distribution in time—evolution. A species as it increases in age, expands its area, while a genus increases its number of species, the younger occupying smaller and smaller areas, usually within the area of the first species, until that becomes very large (and sometimes even then)'. There have been various interpretations of the hollow curve different from that given by Willis, and some criticisms of its validity as used by him.

In attempting to evaluate Willis's contributions to phytogeography one must take account of what is new in his hypotheses and their presentation and also of their defects. Up to a point it is axiomatic that 'age' plays a part in size of ranges and in many aspects of evolution. This can, however, easily be lost sight of and it is to Willis's credit that he brought it to the fore. He has also demonstrated very clearly that a great deal can be learnt by full and careful analyses of published floras and taxonomic monographs. In pointing out that a very large proportion of species, in many genera and many floras, with limited ranges are of recent origin Willis is probably correct, that they numerically completely and invariably swamp old species that are restricting their ranges is more doubtful. The implications of dichotomous divergent mutation would repay more investigation and some of Willis's suggestions, as in chapter VIII of *The birth and spread of plants*, 1949, at least require very careful consideration. Possibly Willis's great service to the studies of plant geography and evolution are rather the stimulation to thought and research in the questions he raises than in the answers he gives. This is particularly true of chapters 10 to 14 of *The course of evolution*, 1940.

Criticisms of the age and area hypothesis and its extensions have been numerous. They include complaints of the absence of adequate definition of terms such as 'allied', 'affinity', and 'old'; that some of his statements are contradictory; that he frequently does not keep to his own rules; that it is exceedingly difficult, if not impossible, for anyone else trying to apply the rules to do so; that relevant data from palaeobotany, particularly for the angiosperms, are so meagre that complete ranges for many taxa cannot be known; doubts that heterogeneous environmental influences cancel out to anything like the extent postulated; the crudeness of Willis's concept of mutation; the inflexibility of many of his claims; and the difficulties of the style, particularly in his first book, *Age and area*.

It is doubtful if Willis kept abreast with modern work in the study of evolution, adaptation, natural selection, mutation, or cytogenetics. Had he done so, some of his extreme, and often very dogmatic, statements would have been eliminated or much modified. That he had a very wide knowledge of some branches of taxonomy is certain but, apart from his own researches on the flora of Ceylon and on the Podostemaceae, most of this was second-hand and he made little or no attempt to test the validity of his sources. So long as his analysis of the work of others supported his hypothesis he was content. Nevertheless, his papers and books are valuable above all because again and again there are in them suggestions for phytogeographical and taxonomic research by modern synthetic methods. It is true that some of the questions he raises, directly or indirectly, have been adequately answered with results different from those given by him. There are, however, many that could form the basis of a life-time's investigation.

Though Willis will probably be best remembered by his extremely valuable *Dictionary of the flowering plants and ferns* (to use the title of the last edition) and by his publications on 'age and area' and the extensions and applications of this concept, it should not be forgotten that he was energetic in matters concerning tropical agriculture and horticulture. As director of the botanic garden in Peradeniya and later of that of Rio de Janeiro he had much to do with cultivated plants. His editorship of sixteen volumes of *The Empire Cotton Growing Review* and his book *Agriculture in the tropics*, of which two editions appeared, testify to his interest in and knowledge of certain aspects of applied botany.

The Council of the Linnean Society of London awarded a Darwin-Wallace silver medal to Dr J. C. Willis, very shortly before his death. The medal was presented to a granddaughter of Dr Willis at a special meeting of the Society, held in the lecture hall of the Royal Geographical Society, on 15 July 1958 in connexion with the Darwin-Wallace centenary celebrations.

W. B. TURRILL

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